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ROY WALDO MINER

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GENERAL

SECTION OF GEOLOGY AND MINERALOGY

DEEP STRUCTURES AND ULTRAMETAMORPHISM*

By E. H. KRANCK**

That metamorphism increases with increasing depth was pointed out by Sederholm (1891) and Van Hise (1899). In 1904, Grubenmann specified three metamorphic depth-zones, an upper, a middle, and a lower, which he later called the Epi-, Meso-, and Katazones. The next step was the introduction of palingenesis (Sederholm, 1907), remelting in the deepest parts of the continental crust. The theoretical discussion of these theories has, during the last half century, overshadowed, the field studies of ultrametamorphic*** rocks.

In particular, structural studies have played a comparatively small role, although the influence of metamorphism on structures was known by the founders of the theories of metamorphism. In addition to the observations made at the beginning of the century, very little progress could be seen before Wegmann around 1930 introduced the ideas and methods of the alpine school into Precambrian geology.

This paper is an attempt to summarize some recent structural studies in deeply eroded areas, mainly from regions of which I have personal knowledge.

Some time ago I published a brief study about paracrystalline metamorphic structures as functions of the physical properties of the rock (Kranck, 1953a). The same problem, with reference to regional conditions, was discussed by Noe-Nygaard and Berthelsen (1952) in an interesting paper on the Nagssugtoc complex in West Greenland.

In order to illustrate the relationship between deep metamorphism and structure, we shall compare some areas from which structural information is available.

For several reasons I wish to mention first the gneisses of West Greenland. The Nagssugtoc area consists of garnet-pyroxene gneisses with layers of marble. They have a nearly horizontal or slightly undulating foliation parallel with the original bedding indicated by the limestone layers. The general structure with open folding is shown in the stereogram (FIGURE 1).

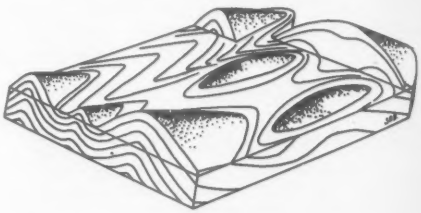

FIGURE 1. Schematized stereogram showing the structure of the gneisses of the Nagssugtoc complex in West Greenland. After Noe-Nygaard and Berthelsen.

The granulite-gneisses are, according to Noe-Nygaard, overlain by strongly granitized gneisses belonging to the amphibolite and epidote-amphibolite facies with very complicated steep folding. According to the authors, granitic material has been transferred from the granulite gneisses to the overlaying strata causing an increase in volume and a complicated folding of the gneiss. The granulite has been degranitized (Ramberg) depending on thermal dissociation of the hydrous minerals and migration of alkalies. This explanation agrees with the opinion of Eskola (1952), Scheumann (1935), Rama Rao (1945), and others.

*This paper, illustrated with lantern slides, was presented at a meeting of the Section on February 1, 1954.

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***In accordance with Sederholm, Niggli, and others, I call a rock ultrametamorphic if it has reached the ultimate stage of metamorphism, where both the chemical and structural properties of the rock have been completely altered through granitization or the beginning of refusion.

With this interpretation in mind it is interesting to make some comparison with other areas. The classic granulite area of Saxony, studied by Scheumann (1935) and others, forms a window overlying Paleozoic rocks. It has an irregularly rounded outline. The overlying rocks are micaschist. Between the granulites and the micaschists there is a narrow zone of tectonized orthogneisses (Rotgneiss). The foliation in the granulites is strong and mostly horizontal or low angle. The rocks are strongly tectonized and often grade into granulite.

Very similar conditions are found within the large granulite area of Finnish Lapland studied by Eskola (1952), Sahama (1936), and myself (1936). Here, also, the general structure seems to be dome-formed, with dominantly flat foliation resembling a primary bedding. The internal deformation is very strong and dragfolds are highly flattened. Strongly paracrystalline granulite, hypersthene gneisses and noritic layers compose the extremely monotonous bedrock of the complex. A large anorthosite area is found at the north end. The overlying rock formations are everywhere removed by erosion and the relationship between the granulites and the surrounding bedrock is not clear. On the north side the granulites seem to grade into lower grade metamorphic migmatites.

The similarity between these areas and the Greenland rocks is in the flat lineation.

Within the Canadian Shield, gneisses belonging to the granulite facies are very common, particularly in the Grenville, where they often are associated with anorthosites. Very little is known about their extension and structure. Osborn's studies (1936) in the Shawinigan area indicate open folding. Low angle foliation is also common in the granulites north of Montreal.

Blake has described granulitic gneisses from the north shore of Lake Athabaska, where open folding and subhorizontal foliation are typical structural features (Blake, 1952).

The charnockites and granulites of the Malabar Metaxis in India seem to resemble the Grenville and, judging from pictures, the type of deformation is partly the same (Rama Rao, 1945). I have no information on the African granulite gneiss areas, or about the granulites of Antarctica and Brazil.

Even if the simple structure described above is not found in all of them, it seems to be a very common feature in that kind of bedrock.

I am going to refer some amphibolite facies gneisses, which also have a low angle main-structure, to a second group of gneiss complexes. In this group, however, we also have big recumbent folds and thrust-folds, and complicated small structures of the type referred to as wildfolding. The last-named resemble the same structures found in tectonized granulite gneisses but are of much greater dimensions, pointing to greater rigidity of the folded material. Granitization with pegmatite and aplite injection plays a very great role, but homogenous granite is lacking.

Gneiss belonging to the second group comprises the main part of the Clyde area in northeastern Baffin Island opposite the granulites of West Greenland mentioned earlier (Kranck, 1953a). The main structure is characterized by a subhorizontal undulating foliation which, occasionally, has been folded in big recumbent folds. The detailed structure is very complicated, due to granitization. The foliation is partly parallel with an old bedding indicated by marble layers and partly a flowbanding along former slip planes. South of Cumberland Sound the gneisses are replaced by charnockitic rocks without trace of deformation (personal communication by Riley). These gneisses may

correspond structurally to the horizon on top of the granulite gneiss horizon of West Greenland.

In two important papers, Wegmann has described the structural features of the gneisses of Ivigtut in South Greenland (1938) and the Ella Ö region in northeast Greenland (1935). The main style of folding in the latter (FIGURE 2). shows the same soft, recumbent type as in the Clyde complex with dominantly horizontal foliation. Ivigtut has steeper folding, in part, but still mostly low angle foliation. Perfect, horizontal foliation is also found in Liverpool Land north of Scoresby Sound. The basal rock, from which the migmatite gneisses in East Greenland have been formed, are the Eleonora Bay sediments (Late Precambrian). These are also found in the same parts of Greenland as unmetamorphosed rock.

The immense areas in the eastern Arctic, occupied by almost horizontally foliated migmatic gneisses, are very interesting, particularly as we find in the same areas undisturbed sediments of great thickness from young Precambrian upward. Is the horizontal foliation a relict bedding, and are the gneisses much younger than has been assumed?

There may also be another side of the problem. It is quite possible that the same main style of structure will prove to be just as common in the southern part of the Shield, but it is impossible to identify it there on account of too few outcrops. We have, for instance, all along the east coast of Hudson Bay and James Bay, migmatic, highly granitized gneisses which in single outcrops show the same structural pattern as the Arctic gneisses mentioned above. For topographic reasons, however, the structure cannot be analyzed on a large scale without very detailed mapping. Thus, in all ultrametamorphosed rocks folded at very great depth, we may possibly have a tendency to form subhorizontal structural patterns.

We have, finally, often connected with the foregoing type, a third type of migmatic gneisses in which remnants of supercrustal rocks with preserved primary structures occur together with highly granitized gneisses and pure intrusive granite. All grades of metamorphism may be found. The basal rocks show steep folding for the most part and, in general, great variarion in structural pattern. Less granitized rocks are often intruded by diapirlike bodies of rejuvenated granitized material (Wegmann, 1935, 1938, Eskola, 1949). The supercrustal rocks may form narrow bands with almost vertical schistosity of arc- and domelike patterns with less well defined general directions.

Sederholm's classic localities in South Finland belong mostly to this type. The structures show a combination of different styles from high plastic to ruptural. The boundaries of whirl-formed "domes" are interesting (FIGURE 3). They consist of ring-formed layers of schists or gneisses with a center of migmatic granite and seem to form in the boundary zone between less

FIGURE 2. The style of folding of the gneisses of Kempe Fiord, East Greenland. After Wegmann.

and more plastic layers. We may find them, also, in gneisses belonging to the granulite facies, as is the case of a complex northeast of the classic Orijärvi region of Eskola (Figure 4 in Parras, 1946).

The Canadian Shield migmatic gneisses of this third group are particularly common in the Keewatin. Bands of pillow lavas and sediments with steep dip and axial plunge may represent steep synclines in the root zone of old mountain chains. It is fairly evident—in most of the cases that I have seen, at least—that the vertical plunge and vertical lineation is a secondary feature formed in connection with the last, strong tangential compression of the rocks. In many cases, detailed investigation proves that the main position of the folding axes within the band of supercrustal rocks was subhorizontal. I want to refer, in this connection, to studies by Simonen and Kaitara in the Tammerfors area in central Finland (Simonen and Kuovo, 1951). A transitional zone between gneisses with steep secondary foliation and lineation, and low angle bedding is exemplified by the Aillik area in Labrador (FIGURE 4).

A more plastic style with arcs and domes is well exemplified by the excellent structural studies in northern Manitoba of Kallioskoski (1952) and Harrison (1949).

These random examples may serve as a basis for a general discussion of the genesis of the gneiss structures in different depth zones.

Any theory about the granulite gneisses

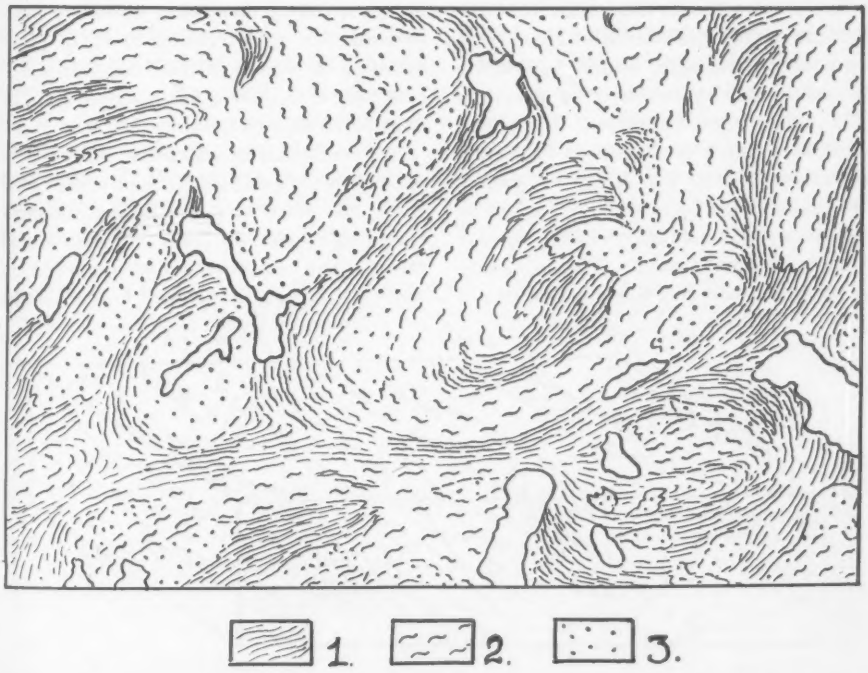

FIGURE 3. Structural map of pyroxene gneisses in Southwest Finland according to Parras. The cyclonelike domes formed by intruding plastic material from below are characteristic. (1) Pyroxene-garnet-cordierite gneisses, etc. (2) Pyroxene diorite. (3) Granite.

has to explain the following points: (1) the anhydrous mineral composition; (2) the simple open folding; (3) the strong tectonization and paracrystalline texture; and (4) the association of granulites with anorthosites, homogenous enderbites, and charnockites.

The examples from Greenland show that the simple structure may be due to a primary, undisturbed composition of the sediments from which the gneiss has been formed. The detail structure indicates that the deformation took place partly in a stage of complete fluidity and partly under a pressure surpassing the yieldpoint of the rock. The vertical stress component in the zone of the earth's crust where the tectonization took place evidently was so strong that vertical displacements of the strata hardly occurred at all. The movement in the rock was mainly subhorizontal flow. This flow could be connected with movements in the undercrust as assumed by Ampferer (1906), Weininger, Meinestz, and Hess (1938), or major deep-thrusts as suggested by Wilson (1949). The granulite gneiss zone, according to such an interpretation, represents intracrustal flow zones between layers of different tenacity. They also may be connected with very deep overthrusts of alpine type. (As is known, we do have in all the great nappes in the Alps a lower flow zone with subhorizontal foliation and an upper part with steep folding of generally recumbent type). The fact that the composition of the granulite gneisses consists of so-called high temperature minerals without hydroxiles, does not in itself prove a very high temperature, as Yoder's experiments have shown. It is mainly the result of lack of water. On the other hand, a removal of water in regional scale is hardly possible under other conditions than heating in the katazone of the earth.

The most convincing evidence of the deep-seated origin of the paracrystalline granulite gneisses is the connection with anorthosites and related plutonic rocks. Whatever our opinion about them, we have to assume a slow crystalization in great depth as the cause of their peculiar structure and, as Ramberg has pointed out, the relatively basic composition (1952).

The conditions at which the second type of migmatic gneisses formed were still nearly the same as the foregoing, although the temperature was lower and the vertical stress less dominant. The presence of water is important and may be one of the

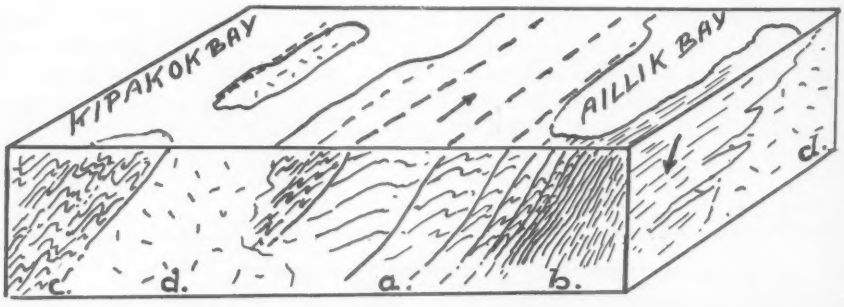

FIGURE 4. The structural relationship between a slightly folded superstructure and "granitized" gneisses with steep linear schistosity. From Aillik, Labrador. (a) Sediments of the Aillik Series. (b) Sheared gneisses. (c) Hopedale gneiss with flow folding. (d) Intrusive granite.

reasons why we have a great difference in plasticity between basic and felsic material (Kranck, 1953a, 1953b) giving rise to agmatic breccias and similar disharmonic structures. Domelike or diapirlike rise of material to overlying, less plastic zones is the result of decreasing vertical stress.

In the third migmatite zone, tangential stress is already dominant and steep foliation is more common than low angle structures. Ruptural deformation and igneous intrusions are typical features. FIGURE 5 shows schematically the different zones in an ideal section formed in a column of undisturbed strata

The superficial structures with folded unmetamorphosed or low-grade metamorphosed strata, the "plissement de couverture" of the French alpine geologists, rests on top of the zones described above The folding here is tight; ruptural deformation dominates over affin deformation. It is interesting to note that we find here also subhorizontal schistosity in connection with big overthrusts. They resemble, so far as the geometrical pattern is concerned, the deep-seated gneisses and in the field it is, therefore, sometimes difficult to ascertain whether a structure represents a nappe of alpine type which has been superimposed by granulite facies metamorphism, or a paracrystalline granulite formed in deep-seated flow-zones.

The structural zones suggested here correspond partly with Grubenmann's epi-, meso-, and katazone but include, in addition to these, the factors of temperature and pressure and also movement. They generally belong to the "Unterbau," the uppermost gneiss zone, and partly to the "Zwischenbau" of Wegmann or "socle" of the French school (Goguel, 1952). For several reasons we rarely find the zones in their typical form or all represented in the same metamorphic column. In the same way as the zones of Grubenmann (1904), the structural zones suggested in this paper represent different steps of alteration. If all zones occur together, they will occur in the order mentioned in the foregoing pages. They will, of course, only roughly coincide with the metamorphic zones, although often enough to be an aid for field studies. Normally, different grades of metamorphism

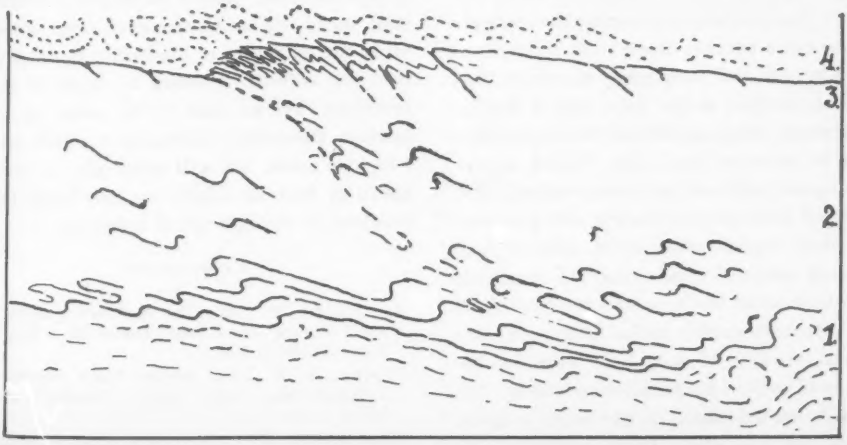

FIGURE 5. Idealized section through a part of the sialic crust showing the main structural zones in the "socle" of a mountainfolding (1), (2), and (3) Socle (4) Superstructure

and metamorphic structures are at least partly superimposed. In certain places we find, therefore, tectonic styles separated in space and in others only in time.

In the areas in south Finland already mentioned it was possible to separate in time an old superstructure with alpine style from a later, superimposed high plastic style combined with granitization. Superstructures may be directly underlain by granulite facies rocks, as in Saxony, or by high plastic amphibolite facies gneisses, as in Greenland. We always have to keep in mind that the conditions at which metamorphism takes place are dynamic and not static.

I have avoided including too much of "granitization" in this discussion, in spite of the fact that it forms one of the most important parts of the ultrametamorphism. From a structural point of view the formation of granitic material is mainly of interest because it will change the physical properties of the rock material. Increase of granitized material will increase the fluidity of a zone and will give rise to intrusions in upper and less fluid layers (Metzger 1947).

The conclusions concerning the genesis of the substructures drawn in this paper are hypothetical, but they have one merit: they can be verified in the field. But it has to be strongly emphasized that this is possible only in suitable localities. I think everyone agrees with me that comparatively few regional descriptions dealing with gneisses and high metamorphic rocks present convincing material concerning the structural development of the area and the genesis of the ultrametamorphic rocks. Generally, this is not because of poor field work but because of lack of cooperation by nature. The deeply eroded parts of the earth's crust, where we can study these problems, are found in the Precambrian shields where the land has in most cases, become a peneplain. This means very little relief and much drift cover. The outcrops are mostly too small and scattered to permit the study of the mutual relationship between main structure and internal structures.

In the foregoing pages I have mentioned a comparatively great number of examples from Arctic North America; this is not because of my personal interest in those parts of the continent, but because they offer the best possibilities for solving the problems of gneisses and gneiss-structures.

Northern Labrador, eastern Baffin Island, and Greenland are recently uplifted edges of the Precambrian shield of North America where the erosion has cut fiord valleys which are unsurpassed as demonstration fields of gneiss structures. But even here, in an area where the bedrock is almost completely exposed, special problems cannot be solved everywhere. One of the main tasks of geological work is to find the right place. I think that this side of geological study has been neglected, except in mineral prospecting. Very much time and good work is wasted in attempts to solve problems where they cannot be solved. We still need some old-fashioned exploring, above all in the solitudes of the North, in order to find the definite answers to many of the problems that we now try to solve by ingenious theoretical deductions which may convince some, but will never give a solid basis of fact on which we can build the progress of the geological sciences.

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SECTION OF PHYSICS AND CHEMISTRY

SOME FUNDAMENTAL LIMITATIONS TO THE PRESERVATION OF THREE-DIMENSIONAL SPECIMENS FOR THE ELECTRON MICROSCOPE*

By THOMAS F. ANDERSON**

For a chemist, the electron microscope, as an instrument, has many limitations; even for specimens of the highest contrast its resolving power is only 10 Å, so we cannot use it to determine the arrangement of atoms in molecules. Moreover, until now, the microscope cannot identify chemically what it is looking at unless the substance in question is crystalline when one can get an electron diffraction picture of it. But the worst limitation for biology and much of chemistry, too, is the fact that specimens must be very thin if much internal structure is to be seen. This limitation has the still more serious corollary that specimens must be studied in a high vacuum—volatile materials such as water must be removed.

Since drying is fatal to almost all biological systems, biologists cannot study living material but, rather, must devise means of preparing dried specimens in such a way that they may hope their specimens have as nearly as possible the structure of the live material. In the early days biologists teased and shredded cells and looked at the air-dried fragments. This was quite satisfactory for some limited purposes but the loss of information in these procedures was tremendous. Not only had the fragments lost the relative positions they had had in the cell, but the surface tension of the evaporating water flattened them and increased the chaos in the specimen.

Two technical developments have overcome these difficulties in recent years. In one, the classical procedure of fixing, dehydrating, imbedding, and sectioning the material has been refined until today it is of great usefulness in electron microscopy of tissues and the like. The relative positions of structure are thus largely preserved and the surface tension artifacts previously mentioned do not play a role because phase boundaries do not travel through the specimen in this type of preparation. In the extremely thin slices of material prepared in this way, however, it is frequently difficult to recognize objects from the thin fragments one is observing, or to appreciate spatial relationships between them. When the objects under study are already small enough for sufficient penetration of the electrons, there seems little point in cutting thin sections of them.

The second type of development eliminates surface tension artifacts in such a way that the specimen retains its three-dimensionality. Two ways of doing this have been devised. In one,[1] the material is fixed and dehydrated by replacing the water by a liquid such as CO_2 under pressure which has its critical point at 31° C. Then, when the temperature is raised above the critical temperature where two fluid phases cannot coexist, the ambient gas is allowed to escape; no surface of a phase boundary can then wreck the specimen. Another procedure,[2] involves rapidly freezing the specimen and then pumping off the water vapor from the essentially solid specimen at low temperature. In this case, the phase boundary between ice and water vapor passes through the specimen, but still it somehow retains much of its three-dimensionality,

*This paper, illustrated with both conventional and stereoscopic lantern slides, was the first of two papers in a symposium on Some New Biophysical Techniques presented at a meeting of the Section on February 2, 1954.

**Johnson Foundation, University of Pennsylvania, Philadelphia, Pa. These studies were aided by a contract between the Office of Naval Research, Department of the Navy, and the University of Pennsylvania, NE 135–197.

although perhaps not so well as a specimen dried by the critical point method.

The latter methods are particularly useful for the study of small objects such as viruses and bacteria. In electron micrographs, one can see that the principal features of their structures are preserved. Other features are not. For example, we know that before drying, the bacteria were floating freely in the medium, but after drying they all adhere in one way or another to the membrane which is used to support the specimen. Moreover, the flagella of bacteria, those whip-like appendages by which they swam through the medium before drying, are usually seen, as in FIGURE 1, to be tightly stretched between the bacterium and the supporting membrane after drying. On the other hand, the shapes of the bacteria themselves and other fragile structures are preserved. I should now like to discuss the sources of these and other artifacts observed in specimens from which the liquid water has been removed and replaced by a vacuum. Here, the principal source of artifacts is the thermal energy associated with each degree of freedom in the specimen. Translational, rotational, and vibrational degrees of freedom play different roles in producing artifacts in the objects an electron microscopist would study. We shall consider each of these in turn in considering "fundamental" sources of artifacts and omit any discussion of "technical" artifacts which depend on *how* the ambient liquid has been removed from the specimen.

Translation and the Effect of Gravity

Let us assume, at first, that our specimen consists of a dilute aqueous suspension of spheres of various diameters which is contained between two horizontal membranes a given distance apart, say 1 mm. Let us assume further that the spheres have the same density as the water and that the water is removed instantaneously and perfectly; *i.e.*, at the magic instant of dehydration, only the water molecules disappear from between the membranes. Three important parameters of the system will

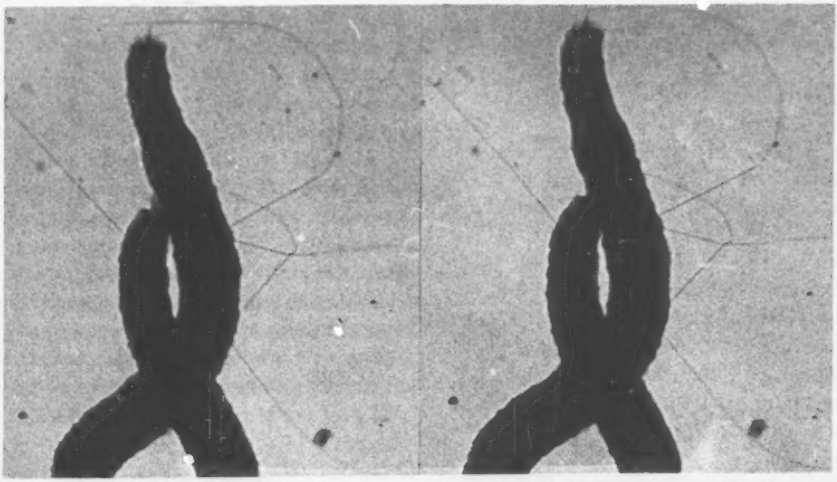


FIGURE 1. Stereoscopic electron micrograph of bacteria (spirilla) prepared by the critical point method. Note that the helical structures of the cells are preserved, but that the flagella are tightly stretched in space from the cells to the supporting membrane on which the remainder of the flagella lie flat. Magnification × 41,000, stereoscopic angle 10°.

change at this instant; its dielectric constant drops to unity, and its density and viscosity drop to zero. Now the particles will fall freely in the earth's gravitational field and they will also move freely in random directions with their thermal energy. We may assume, as a first approximation, that a moving particle in a vacuum will stick to anything it hits for, after the dielectric constant has dropped to unity, the ionic double layers which had stabilized the aqueous suspension no longer exist.

Let us first see if there is any chance of studying these free particles with the electron microscope in the absence of gravitational fields. To do this we may note that each translational degree of freedom will have the average kinetic energy:

$$\tfrac{1}{2} m \overline{v^2} = \tfrac{1}{2} k T \qquad (1)$$

where k is Boltzman's constant (1.36×10^{-16} ergs/degrees), T is the absolute temperature, m is the mass of the particle, and $\overline{v^2}$ is its mean square velocity in a given direction. From this relation it may be estimated that a sphere of unit density 3000 Å in a diameter moves about 1 cm. per second, while a 1 gm. weight would move 20 Å per second at 300° K in the absence of gravitation. Since an exposure time of about 10 seconds is required for the electron microscope, we could not expect to obtain a picture of a 1 gm. particle in free space at 300° K, which is sharper than about 200 Å. We could lower the temperature but, even at absolute zero, we should be limited in resolution, for the Heisenberg uncertainty principle states that in determining the position of a particle to within a resolution, Δx_1, we should produce an uncertainty in its momentum, $m\Delta v_x$ such that

$$m\Delta v_x \cdot \Delta x_1 \geq h \qquad (2)$$

where h is Planck's constant (6.6×10^{-27} erg · sec). But during the exposure time, t, the particle will have moved a distance $\Delta x_2 = \Delta v_x \cdot t$ so that we get

$$\Delta x_1 \cdot \Delta x_2 = O(ht/m) \qquad (3)$$

for the order of magnitude of the product of the two uncertainties in position. This product could be as small as we like if we could make the exposure time as short as we like, but space charge effects, in the electron microscope, make exposures of the order of seconds necessary if a picture of high resolution is to be obtained. It turns out that a 10-second exposure of a particle 3000 Å in diameter initially at 0° K could give only about 150 Å resolution if our microscope were perfect in all other respects.

Returning to the surface of the earth and room temperature, let us see for what sized particle gravitation is important in determining on which of the membranes the spherical particles in our sample will come to rest. The height, h, to which a particle of vertical kinetic energy $\frac{1}{2}kT$ will rise against the gravitational field, g, is related to its mass and temperature by the expression

$$m g h = \tfrac{1}{2} k T \qquad (4)$$

for at the top of its trajectory its kinetic energy will have been transformed into potential energy ($m g h$). Thus at 300° K the average particle of mass 10^{-15} gm. (10^{-5} cm. diameter) will be able to rise 0.02 cm. against gravity while a particle of mass 10^{-12} gm. (10^{-4} cm. diameter) can rise only 200 Å. The size of particle at which gravitational forces begin to determine distribution is in the middle of the range of the electron microscope. The larger viruses have this mass, but most bacteria are 1000 times as heavy so that almost all bacteria would fall to the lower membrane when the density and viscosity of the ambient fluid vanish.

Thermal Rotation

Now we might be interested in knowing if the relative *orientations* of the particles

might not be preserved. Will particles rotate before landing and sticking on the membrane? Here again each degree of rotational freedom will have an average energy $\frac{1}{2}kT$. Assuming that the particles are spherical we can estimate the rate of rotation about a particular axis from the equation

$$E = \frac{1}{2} k T = \frac{1}{2} I (2\pi n)^2 \qquad (5)$$

where I is the moment of inertia of the sphere ($2mr^2/5$) and n is the number of revolutions it makes per second. It is seen that a sphere 10^{-3} cm. in diameter would make 1 revolution per second at 300° K and since it would require only 1.4×10^{-2} sec. to fall 0.1 cm. it would land with orientation essentially unchanged. On the other hand, a particle 10^{-4} cm. in diameter rotates about 10^3 times a second, so it would make about 10 revolutions before landing. Orientation would not be preserved for particles as small as bacteria. Small particles would be spinning at a fairly rapid rate as they land.

Some particles like bacteria and bacteriophages have relatively light projections on them and we have been interested in knowing what the chances would be that the projection would strike the membrane first. In FIGURE 2 an analogous problem is stated mathematically and its rather complex solution, as worked out with Doctor David Sayre, is given there, too. Of particular interest is the solution for V/R = 0.63 which, at thermal equilibrium, is the ratio of the mean translational velocity of the sphere in a particular direction to the mean rotational velocity about a particular axis which a point on its surface would have. When V/R = 0.63 and the projection extends four or more radius-lengths from the center, the projection will always strike the plane first. Thus the tip of the tail of a tadpole-shaped bacteriophage particle might have an excellent chance of striking the supporting membrane first. Whether the head would strike subsequently would depend on many factors including the stiffness of the tail, the rigidity of its attachment to the membrane, and the angle the tail made with the membrane when it struck.

Thermal Vibrations

So far we have shown that the translational velocities and rates of rotation of unattached particles are so great that they cannot be studied in the electron microscope. It is only after they have become attached to the supporting membrane that high resolution pictures can be obtained of them. We have now to consider how thermal vibrations will subsequently affect the configuration. We can deal theoretically with the previously mentioned artifact of the taut flagellum.

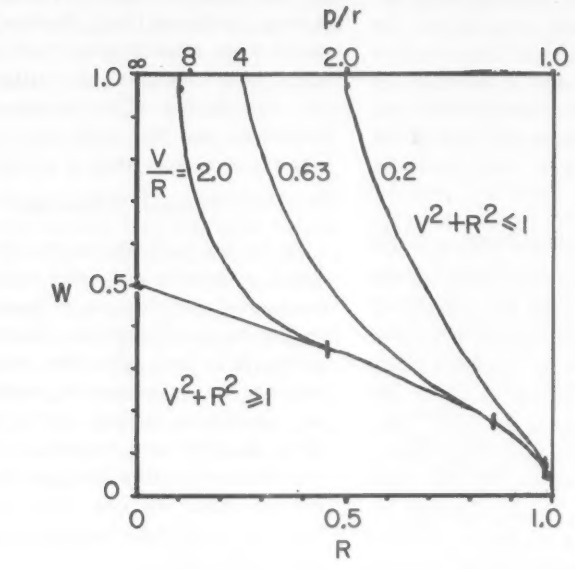
Suppose the flagellar material weighs ρ gm. per cm. and is stretched between two objects ℓ cm. apart with a force, F. Such a fiber has many normal modes of vibration whose frequencies are given by the equation

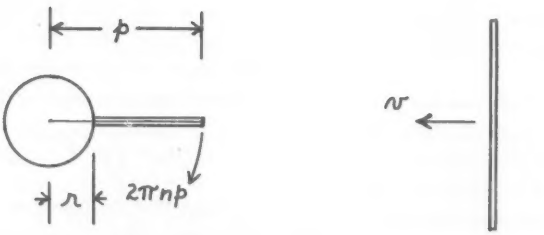
$$f_i = \frac{i}{2\ell}\sqrt{F/\rho} \qquad (6)$$

where i is an integer. Each of these normal modes will have an average total energy, kT, half of which (on the average) is in the form of kinetic energy and half in the form of potential energy. The latter is proportional to the square of the *displacements* of the various parts of the fiber from their equilibrium positions. If we sum the mean square displacements over all the modes, one obtains[3] the mean square displacement, $\overline{u^2(x)}$, of any point, x, measured from one end of the string:

$$\overline{u^2(x)} = \frac{kTx(\ell - x)}{F\ell} \qquad (7)$$

The expected values of the fundamental frequencies and mean displacements of a





PARAMETERS: $V = \frac{v}{2\pi r n p}$ MOVING PLANE

$$R = \frac{r}{p}$$

(A) If $V^2 + R^2 \geq 1$, $W = \frac{1}{\pi} \cos^{-1} R$

(B) If $V^2 + R^2 \leq 1$, $W = \frac{1}{2\pi}\left[\sin^{-1} V + \cos^{-1} R - \frac{R}{V} + \frac{1}{V}\sqrt{1-V^2}\right]$

FIGURE 2. The probability, W, that a projection extending p cm. from the center of the disc of radius, r, will strike a plane first if the plane is moving toward the disc with a velocity, v, and the disc makes n revolutions per second. There are two solutions (A) and (B) that merge when $V^2 + R^2 = 1$.

flagellum 2 μ long are given in TABLE 1 for various tensions. Since we do not see Brownian motion in such flagella, we must conclude that the tension, F, must be at least 10^{-5} to 10^{-4} dynes corresponding to a stress of 3×10^6 to 3×10^7 dynes/cm.^{2} The tensile strength of a dried flagellum must, therefore, be at least this large.

To appreciate how such tensions arise, it is instructive to note from TABLE 1 that the flagellum waves around with enormous amplitudes for low tensions. If it strikes something it is apt to stick to it, and if it sticks to a point which is *not* on its position of equilibrium its tension will increase. As a result of this increased tension, its amplitude will be decreased; this, in turn, decreases its chances of encountering another object. As it continues to strike new objects the tension finally becomes so great that the amplitudes required to strike another object becomes prohibitive or the flagellum breaks.

We can analyze this situation from a statistical mechanical point of view using the model illustrated in FIGURE 3. Here a flagellum is rigidly supported at one end and at the other end it adheres to a rough supporting membrane with a distance, D, between elevations. Assuming that the flagellum makes an average angle, θ, with the membrane, we should like to estimate the number of times per second, ν, that its amplitude at X = D will reach or exceed D tan θ when it will touch the next elevation and stick to it.

TABLE 1

EFFECT OF BROWNIAN MOTION ON VIBRATION OF A FLAGELLUM 20,000A LONG AND 100A RADIUS AT 300° K.
($\rho = 4.7 \times 10^{13}$ gm./cm.)

Tension (dynes)	*Stress, S (dynes/cm^2)*	*Frequency of fundamental mode, f_1 (sec^{-1})*	*Mean displacement at center $\{\overline{u^2(\ell/2)}\}^{1/2}$*
10^{-2}	3.2×10^9	10^8	1.4 A
10^{-4}	3.2×10^7	10^7	14
10^{-6}	3.2×10^5	10^6	140
10^{-8}	3.2×10^3	10^5	1400
10^{-10}	3.2×10	10^4	14,000

An approximate solution can be made by setting ν equal to the product of the principal mean frequency of vibration, f_D, of the point x = D, times the probability, W, that the amplitude will then be at least as great as D tan θ. For the characteristic frequency of the point x = D, we take that of the mode, $i = \frac{i}{2D}$, which has its antinode at x = D, setting

$$f_D = \frac{\ell}{4D}\sqrt{F/\rho} \qquad (8)$$

To estimate W, we assume that the potential energy will have a Gaussian distribution about $\frac{1}{2}kT$. Then

$$W = \frac{2}{\sqrt{\pi}} \int_s^\infty e^{-t^2} d\,t \qquad (9)$$

where s = D tan θ /$(2kT\ D/F)^{1/2}$. The reciprocal of the frequency, $\tau = \frac{1}{\nu} = \frac{1}{f_D \cdot W}$, will then be the mean time for the displacement to reach D tan θ and we obtain after integration of EQUATION 9,

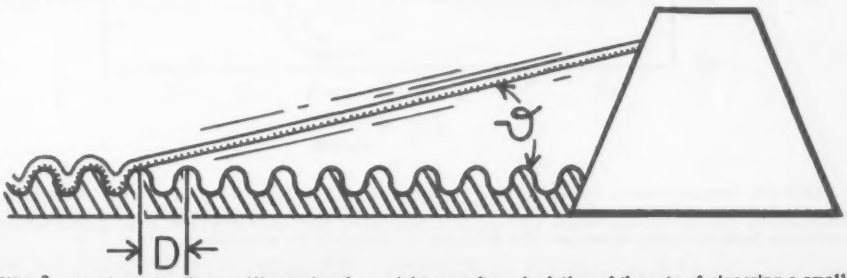


FIGURE 3. A schematic diagram illustrating the variables in the calculation of the rate of zippering a small fiber onto a rough surface.

$$\tau = 4 \tan\theta \left(\frac{\pi \rho D^3}{2kT}\right)^{1/2} \cdot e^{\frac{DF \tan^2\theta}{2kT}} \qquad (10)$$

In FIGURE 4 the values of τ obtained from this expression are plotted against F on a logarithmic scale for various values of D and τ together with values from an expression obtained by Mr. Marvin A. Weinstein[4] who made a much more rigorous derivation, but one in which it was difficult to see the physical significance of certain simplifying mathematical assumptions. Since the same exponential term dominates both expressions, however, they agree very well. The shapes of the curves indicate that the rate of zippering of the flagellum to the surface is extremely rapid when D F $\tan^2\theta$ is small and then practically stops as this product reaches a critical value. In FIGURE 5, the tensions in this metastable

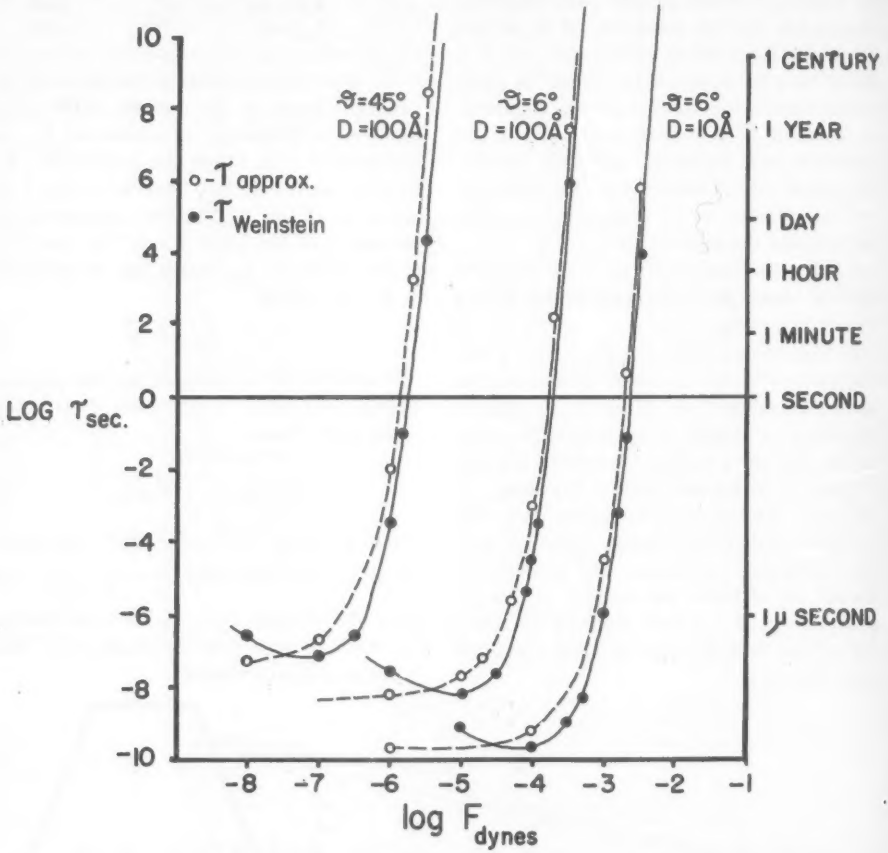


FIGURE 4. The logarithm of the mean time, τ, required for a fiber like that in FIGURE 3 with $\rho = 4.7 \times 10^{-13}$ gm. cm.) to make contact with the next elevation on the surface at 300°K as a function of the logarithm of the tension in the fiber for various values of θ and D. Values calculated from EQUATION 10 are given in open circles while those in closed circles are from Weinstein who obtained the expression:

$$\tau = \frac{2\sqrt{2\pi k \rho D}}{F \tan\theta} \cdot e^{\frac{FD}{2kT} \tan^2\theta}$$

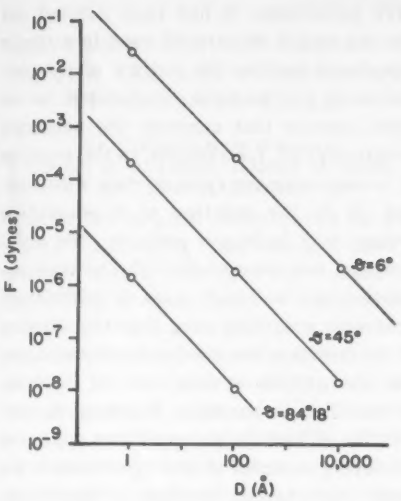


FIGURE 5. Values of the "equilibrium" tension, F, developed in a fiber zippering onto a surface as a function of its roughness, D. The values are calculated for $\tau = 10^6$ sec. as the time required for the fiber to make contact with the next elevation.

condition are plotted against the roughness, D, for various values of the angle θ. It is evident that these tensions increase rapidly as the surface becomes smoother and as the contact angle decreases.

Now, in stereoscopic pictures of specimens dried by the critical point method, we have observed flagella making angles as low as 15° with a clean formvar membrane. Estimating the roughness, D, of such membranes to be about 40 Å one obtains 10^{-4} dynes as an approximate value for the equilibrium tension in good agreement with the previously noted fact that the Brownian motion is not observed in them.

It seems likely that similar considerations would apply to the zippering together of many types of structural elements such as those observed[5] between ghosts of red cells and the supporting membrane.[6] In each case it seems likely that a metastable equilibrium is rapidly established in which the tensions and stresses are so high that Brownian motion is seldom seen. This is fortunate, for otherwise sharp pictures of such specimens could not be obtained with the electron microscope.

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PHYSICAL LIMITATIONS TO SENSITIVITY IN THE RECORDING OF RAPID ENZYME REACTIONS WITHIN LIVING CELLS*

By BRITTON CHANCE**

It has recently been possible to devise an explicit formulation for the optimal design of rapid flow apparatuses employing spectrophotometric observation. This equation incorporates chemical, geometrical, optical, and electronic factors, and shows the proper weight that should be given to these factors in order to obtain optimal performance. The general results of this derivation emphasize the importance of photoelectric surfaces of optimal sensitivity, and light sources of maximal intensity (steradiancy). Geometric factors are interrelated with optical factors in such a way that the aperture of the observation chamber is made consistent with that of the optical system, and the derivation emphasizes the desirability of using large area optics and observation chambers, and of utilizing as much of the solid angle of the light source as possible. The chemical factors emphasize the importance of measuring at a wave length where the molecular extinction coefficient is a maximum, because this quantity is the only ferm in the equation that appears with a power higher than the first.

The performance of the method increases linearly with the amount of material that can be expended in a single experiment. Since this quantity is unfortunately limited by the availability of biological materials or by the small yield of laborious preparative procedures, it has been pointed out that the weight of material used in a single experiment implies the product of its concentration and the total volume used. In enzyme systems that consume the substrate completely, as, for example, in the reaction of a respiratory enzyme system with oxygen, or in the reaction of a peroxidase system with hydrogen peroxide, the solution that has passed through the flow apparatus may be used again in further experiments providing only that the dilution of the solution has not been excessive, and that the enzyme system has not been inactivated. It is possible, therefore, to connect the effluent from a rapid flow apparatus employing systems of this type back to the input, and thereby maintain a continuous regeneration of the reactants, and an effectively large increase of the volume of reactants available for a single experiment.

An apparatus has been designed in accordance with this principle, and is called the Regenerative Flow Apparatus. The effective volume of reactants available is increased about 100 times in this particular design. Because of this increase in performance, it has been possible to record the spectrum of sequences of respiratory enzyme systems of intact cells in the course of their reaction with oxygen. A clear separation between the speeds of oxidation of cytochromes a_3, a, and c from those of cytochromes b, flavoprotein, and reduced pyridine nucleotides are obtained, showing that the respiratory enzyme system may be divided up into two rather different kinetic groups: (1) a closely integrated oxidizing system, which carries oxidizing equivalents up through cytochrome c; and (2) a diversified oxidizing system that finally delivers these oxidizing equivalents to the appropriate systems of the intact cell.

*This paper, illustrated with lantern slides, was the second of two papers in a symposium on Some New Biophysical Techniques presented at a meeting of the Section on February 2, 1954.

**Johnson Foundation, University of Pennsylvania, Philadelphia, Pa.

SECTION OF BIOLOGY

HERPES SIMPLEX VIRUS OBSERVED IN THE ELECTRON MICROSCOPE IN SECTIONED CELLS*

By COUNCILMAN MORGAN, SOLON A. ELLISON, HARRY M. ROSE, AND DAN H. MOORE**

Since the original observations by Lipschütz[1] indicating that intranuclear inclusion bodies developed in tissues infected with herpes simplex virus a controversy has developed. Did such inclusions indicate actual invasion of nuclei by virus particles or were they an unusual but nonspecific reaction to the infection? In an attempt to answer such questions, infected chorioallantoic membranes were sectioned and examined in the electron microscope.

The HRE strain of herpes simplex was inoculated onto the chorioallantois of 11-day-old chick embryos, and the embryos then incubated at 35°C for 24 to 72 hours. Pieces of membrane were excised, fixed in osmium tetroxide buffered at pH 7.4,[2] dehydrated in ethyl alcohol and embedded in methacrylate. Thin sections were cut on a thermal-expansion microtome (designed by Doctor Keith Porter). After mounting on formvar-covered screens the sections were examined in an RCA type EMU electron microscope.

Previous electron microscope observations, made on ultracentrifuged preparations of infected vesicle fluid and of fluid from the amniotic cavity of chick embryos, had shown particles considered to be virus having diameters ranging from 133 to 233 mμ.† [3,4] It, therefore, was surprising in sectioned nuclei to discover particles, also considered to be virus, with diameters varying from 100 to 130 mμ.[5] Subsequent examination has shown even smaller particles, 40 to 60 mμ. in diameter, aggregated within some nuclei. In the cytoplasm, however, larger particles (130 to 180 mμ.) were encountered. On the basis of many such observations, it seemed likely that the virus of herpes simplex increased in size not only within the nucleus, but also within the cytoplasm of the host cell.

This information had been obtained by examination of relatively thick sections, from which the methacrylate was removed by immersion in amyl acetate. This technique had the advantage of permitting the study of relatively thick sections, where many virus particles were left undisturbed and could be localized readily within infected cells.

On the other hand, because few virus particles were transected in such sections, details of internal structure could rarely be visualized, the virus remaining opaque to the electron beam. Removal of the embedding plastic also produced considerable destruction of cellular fine structure. Fortunately, the virus appeared to possess a remarkable resistance to procedures which generally caused extensive distortion of mitochondria and other cellular components.

To study the fine structure of the virus, very thin sections had to be employed without removal of the embedding material. It was found that sections could be cut which transected all except the smallest viral components, *i.e.*, those less than 60 mμ. in diameter. Study of such sections showed aggregates of spherical particles, 40 to 60 mμ. in diameter. In many nuclei, presumed to represent later stages of infection, these primary bodies were found each to be enclosed by a single dense membrane. Within

*This paper, illustrated with lantern slides, was the first of three papers in a symposium on The Electron Microscope in Biological Research presented at a meeting of the section on February 8, 1954.

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†A recent report (K. Munk and W. W. Ackerman. 1953. J. Immunol. 71:426.) indicated the particle size to be 96 mμ. by sedimentation measurements and an average of 116 mμ. in electron micrographs.

the cytoplasm, however, most of the larger particles possessed a double membrane.

Summary

Comparison of sections treated in the manner described suggested that, within the nucleus, the virus particle of herpes simplex increased in size by the formation of a single membrane about a primary body. After release from the nucleus, a second membrane appeared further to enlarge the virus within the cytoplasm of the host cell.

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A STUDY OF ISCHEMIC MUSCLE*

By DAN H. MOORE AND COUNCILMAN MORGAN**

In an attempt to better understand the factors which cause traumatic shock, mouse and dog leg muscles made ischemic by the application of tight tourniquets have been studied. Light absorption measurements of serum from animals previously injected *in vivo* with T-1824 dye show that after release of the tourniquet the concentration of dye (which is bound to the serum albumin) at first rises, indicating that water is leaving the blood more rapidly than albumin. After 15 to 20 minutes the concentration of dye falls indicating a reversal of rates.[1] Electrophoretic analysis of buffered saline (pH 7.4) extracts of the injured muscle, taken at hourly intervals, indicate that, of the serum proteins, only albumin is found until after four to five hours when small quantities of globulin appear.[2] Electrophoretic analysis of the serum reveal a drop in albumin and γ-globulin (proteins of lower molecular weight) and a concommitant rise in α-&-β-globulins (proteins and lipoproteins of higher molecular weight). Autolysis resulting from the ischemia increases the permeability of the capillaries but does not destroy them. Cooling prevents apparent damage.[3] Electron micrographs of thin sections fixed with osmium tetroxide and embedded in methacrylate show that the ischemia caused deterioration in myofibrillar morphology and a marked increase in the number of spherical bodies measuring from 0.1μ to 1μ. Sections of the larger bodies reveal an internal structure characteristic of mitochondria.

*This paper, illustrated with lantern slides, was the second of three papers in a symposium on The Electron Microscope in Biological Research presented at a meeting of the Section on February 8, 1954. The third paper "The Fine Structure of Mitochondria" by G. E. Palade, had not been received at the time of publication.

The work reported here was supported by the Office of Naval Research, U.S. Navy.

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SECTION OF PSYCHOLOGY

PROJECTIVE TECHNIQUES IN ADVERTISING RESEARCH*

By DARRELL B. LUCAS**

Almost every popular magazine or woman's magazine we pick up is currently carrying a series of advertisements for a baking mix which involves a well-known female character, a well-known product, and a prominent copy theme. I have seen and worked on research connected with these advertisements ever since World War II. About five years ago, the company selling this product spent nearly $25,000 doing research on one single magazine advertisement—mostly quantitative research on the impressions made by the copy.

More recently, the same company has completed three successive major copy tests, all three based on projective techniques and representing three of the leading advertising research companies now specializing in projective methods. I think it is fair to state that the three investigators in charge of these tests arrived at essential agreement on the major conclusions. These investigators differed enormously in techniques and were sometimes in open conflict on details and reasons for arriving at the same conclusions. To be very frank, I do not know just how much these projective studies have influenced top level decisions on the advertisements. It can be said that important aspects of copy are at variance with the research recommendations. My own views are pretty much in agreement with the general conclusions of the three investigators, although their specific recommendations show some lack of understanding of advertising.

If there is anything disturbing about these experiments, other than their enormous cost, it is the dogmatic nature of the conclusions and recommendations presented by each of the three investigators. Sometimes the seriousness of their positive statements becomes a little ludicrous, as when they make some obvious generalized statement which everyone has accepted as a fact from the beginning. One of these operators, for example, recently demonstrated that people who read *Better Homes and Gardens* are interested in improving their own homes and gardens. This discovery cost thousands of dollars.

It is no longer amusing when the same positive tone is exhibited in stating new or controversial conclusions. Often such statements are made on the basis of simple, crude checks indicating, perhaps, a hesitation in associating a word—a word of three or four syllables—which may suffer from unfamiliarity or poor phonetic quality as much as from bad associations or connotations. My purpose is not to concentrate on the use of any specific technique and I shall not go into detail on that point any further.

What a contrast there is between these studies conducted by commercial organizations, employing semiskilled interviewers, dealing with biased samples of normal people, with regard to buying motivation—what a contrast between their extravagant claims and the modest hopes and beliefs of clinical psychologists applying these techniques with their own skilled hands, dealing with problem cases who have come to them for help, and using these techniques to diagnose personalities for the purpose of achieving more effective clinical treatment! I should like to quote just a few significant statements picked up last September at the American Psychological Association meetings, mostly from symposia dealing with projective techniques:

*This paper was presented at a meeting of the Section on February 15, 1954.

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(1) Clinical psychologists have been using projective techniques to make a general diagnosis, but have failed to use them in making predictions or prognoses.

(2) Our applications of projective techniques have been entirely empirical.

(3) We have no evidence that the projective techniques cover a proper selection or sample of the behavior tendencies of the individual.

(4) There is a lack of agreement between projective behavior and social behavior.

(5) There is greater need for standardizing the administering of the Rorschach test than for standardizing the interpretation of the data.

(6) The personality of the examiner affects responses to the Rorschach test among both normal and abnormal subjects.

(7) The conclusions of clinical psychologists depend upon the clinician and the theories which he follows.

(8) We can validate projective techniques only when they are applied to specific situations, under a specific hypothesis and when the measure can be applied to overt behavior.

(9) We must have more than testimonials to support projective techniques.

(10) We have almost finished the first stage in the use of projective techniques, the stage in which we are sure of our ground. We are getting into the second stage, where we are beginning to demand proof of the validity of our methods.

(11) Projective techniques have promise. but the most optimistic claim I can make is that they may predict with a correlation of about plus .30.

These statements are probably representative of the cautious approach which clinical psychologists are using after more than 20 years of frequent application of projective techniques. Yet when you examine the statements closely it is clear that they are not defensive, but rather that they are challenging. Several of the quoted speakers are members of the Society for Projective Techniques and they are not in a mood to abandon the methods as unusable. They simply are demanding the systematic and scientific appraisal of methods which they confidently believe hold greater promise for the future.

May we take our cue from the clinical investigators? These methods, which we are starting to use in advertising, show promise; but they are still unproved even in the clinic where they originated and for which they were designed. We just do not know how valid their application may be in advertising, and certainly it will require considerable adaptation before we can reach the modest level now hoped for in clinical psychology. Nevertheless, many of us are expecting a lot to be gained through the application of projective techniques to advertising research.

Before we go further, it might be a good idea to enumerate some of the projective techniques which we have been using or trying to use in advertising research. Altogether I am going to mention six of these techniques. The first is the method of word associations. One interesting association test dealt with prunes, and the result was that the advertiser tried to eliminate the word completely from his vocabulary. Prunes, which advertising men would like to have you think of as a delicious fruit confection, actually have so many bad associations that practically no one thinks of them as an attractive food. Association tests have shown that the word "promise" also has bad connotations. Many people immediately think of broken promises. There is also a considerable amount of hesitation or blocking in the attempt to give words associated with "promise."

The second projective technique employs sentence completions. This method gives the respondent more substance to develop and possibly more direction. A

survey for a magazine uses the incomplete sentence, "The ads in *Better Homes and Gardens*....." When such a statement is developed by any considerable number of people into an expression of interest in reading advertisements, the people in the business are apt to be carried away. That is especially true if the references are favorable. Probably the advertising references have much more meaning than those obtained from structured questions such as, "Have you ever read advertisements in *Better Homes and Gardens?*" or "How much confidence do you have in the products advertised in *Better Homes and Gardens?*"

The third projective technique already applied in advertising is role playing. Perhaps the most striking publicized example involving this technique was a study of Nescafé coffee. When 50 women were asked to describe a housewife carrying a shopping list including Maxwell House coffee the reports were quite favorable, while the comments by another 50 women on a housewife carrying a list that was identical except that Nescafé was included instead of Maxwell House, were critical. This housewife was looked upon as lazy, as a spendthrift, as a poor wife, and as indifferent to the welfare of her family. Now the advertiser of Nescafé is stressing delicious flavor and uniformity of flavor.

Of course the purpose of this type of test is to reveal the personality and the motives of the respondent. Sometimes the characteristics which the subject attributes to someone else are the opposite of his own. Usually, however, the characteristics are not those about which he is indifferent but either reflect his own feelings or their opposite.

The cartoon technique, in which missing words are to be supplied in a balloon, or as a caption, is a fourth method. A good example shows a waiter in a restaurant bringing a tray and saying, "I'm sorry, but the cook didn't prepare this dish the way you ordered it." The test is to fill in the balloon for the guest, who may say anything from "That's all right–I'll take it." to an outraged "Take it back!"

Fifth, and very promising, are the applications of the thematic apperception test (T.A.T.). This method employs a picture, not a line drawing, and either omits some details or depicts an unresolved situation. One example shows a woman, money in hand, approaching a business location in which a bank and a dress shop are equally convenient. Women were asked to interpret the situation.

Finally, we give some attention to the Rorschach ink blot test. I have heard it said repeatedly that we have no advertising application for the Rorschach test. I believe that applications of the Szondi test are even less likely.

Now, let us turn to current applications in advertising. There is probably more excitement over the use of projective techniques in advertising today than over anything else in the business. The inroads of clinical techniques in advertising research have developed rapidly in recent years, though very slowly over a great many years. The literature of European investigators shows that they have applied psychoanalytic techniques for many years.

Here in the United States, I am inclined to tie this movement to the appearance on the scene of Doctor Ernest Dichter, whose work at the J. Stirling Getchell Agency goes back at least 20 years. Doctor Dichter did research on oval bars of hand soap and Plymouth automobiles, among other products. He used a psychoanalytic approach and his interpretations are Freudian, at least Freudian as we so often interpret it in a looser sense.

Doctor Dichter and others did not expand the influence of clinical methods rapidly until after World War II, and during that time a number of social psychologists

had made significant contributions to survey methods and to copy testing. As a matter of fact, the term social research has come to identify a great deal of work in advertising, also known as motivation research, and is often intended to designate research using projective techniques.

Because the Advertising Research Foundation is developing such a productive program in motivation research, I should like to tell you a little of what the Foundation is and how it works. The Foundation was organized in 1936 by the two dominant advertising associations, presumably as a clearing house for advertising research which would be acceptable to the industry as a whole. Much research in advertising is done by people who have something to sell, and is written up and publicized by people especially gifted in depicting favorable implications. The advertising business assigned the Foundation the task of developing objective research. By 1939 the Foundation had taken over a continuing survey of newspaper reading, and later developed surveys in other fields including car cards, farm papers, business papers, and magazines (in Canada). Some of these activities began to subside by 1950, and the Foundation was reconstituted and opened to subscriber memberships available to advertising agencies, advertisers, and media. Today most of the largest agencies are subscribers, and many manufacturers also subscribe, as do the major publications and broadcasters.

The functions of the Foundation have broadened beyond the supervision of specific research jobs, and now include fundamental research studies and the appraisal of advertising research already done. One of these basic projects is that of the Motivation Committee, very ably headed by a trained clinical psychologist who is also Chairman of the Executive Committee of a prominent advertising agency, William Esty and Company. I refer to Doctor Wallace H. Wulfeck. The Motivation Committee includes members ranging from those completely uninitiated in formal psychology to experienced psychologists.

The first thing the Committee did was to attempt to define motivation research in relation to advertising. Suggestions included references to such things as the "mainsprings of human behavior" and involved such expressions as "why people behave as they do." Whether right or wrong, the Committee promptly agreed that these expressions indicated clearly the area in which it should work, although no attempt was made by the group to develop a formal definition of its function.

The second step of the Committee, both productive and disappointing, was the assembling of a bibliography. It was productive because it produced a long series of titles related to motivation and covering the literature on projective techniques involving applications to advertising. It was disappointing, to me at least, in that it turned up very little literature suitable for uninitiated students or workers in advertising. The average advertising research worker is not trained in terminology to read most of this literature, nor does he have the proper foundation for carrying the clinical applications over into significant advertising research. Nevertheless, this bibliography stands as a highly valuable contribution of the Committee.

Next came a splendid glossary intended as a reading guide for literature in motivation research. This 130-page document—just coming off the press—contains a good selection of terms accompanied by two types of definitions. The first definition of each term is technical and precise. This definition is followed by a second interpretative statement intended to give maximum meaning for intelligent people not familiar with clinical language. While the technical definitions are admirably precise, the freer, popularized descriptions probably exhibit more ingenuity.

In the short space of a year, this Motivation Committee of the Advertising Research Foundation has carried out three additional projects. One of these projects is a directory of commercial research organizations qualified and experienced in advertising research involving the use of psychoanalytic and projective techniques. Whereas we originally thought of perhaps half a dozen concerns in the United States, clearly identified in this category, it develops that there are scores. Some must have interpreted the specifications quite freely while others were in the process of employing personnel to handle such research. Many others had long since demonstrated their competence and readiness to serve.

Next, and not quite completed, is a directory of clinical psychologists interested in brief or extensive ventures into the advertising business as designers of research or advisors. There are actually scores of clinical psychologists, or clinically trained psychologists, who have moved into this area or have offered to apply their knowledge and experience to advertising problems. We welcome this interest on the part of a profession with this specialized training, and hope that it will lead to rapid advances in advertising research.

Perhaps the most spectacular of all the Committee's accomplishments, during its short life, is the sponsorship and critical guidance of the writing of a handbook and textbook of *Motivation Research for Advertising and Marketing*. This 350-page book is being published by McGraw-Hill Book Company and is scheduled for Oct., 1954. It will provide advertisers and advertising research people with a brief discussion of six projective techniques. The illustrative cases are taken from advertising, and recommendations are made for use of the techniques discussed in the advertising field.

Altogether, this Committee has made phenomenal progress and has moved effectively to provide the advertising business with information it has wanted for more than a year. The work has been competently done throughout, and represents the combined knowledge, abilities, and judgments of a group better qualified than any single individual.

In closing I should like to give you some idea of the perspective of the Committee with regard to the whole field of projective techniques as applied to advertising. This I cannot do in an official way, so I am asking your indulgence while I express my own opinions. I believe that these opinions are not in conflict with the thinking of the Committee as a whole; yet I should be taking liberties if I said that these remarks actually represent the Committee's views.

To begin with, all of us realize that the wide application of projective techniques to advertising problems is comparatively new. While Doctor Dichter has done conspicuous psychoanalytic work and has developed a type of depth interview over a period of about 20 years, the attempt to employ role playing or Thematic Apperception Tests is much more recent.

Furthermore, none of us has any illusions as to the proved high validity of any of these techniques in advertising research. The Chicago Tribune points out concerning one of its recent studies conducted by Social Research, "The projective tests were devised by clinical psychology and psychiatry as a means of getting the individual to reveal his own idiomatic way of structuring experience, in order to disclose his private world of meanings, values, feelings, and attitudes." The techniques have not been validated in the clinics for which they were devised, and their application must be even more tenuous in the field of mass motivation which we call advertising.

Another thought which cautions all but the most eager proponents of projective

techniques in our field is the fact that we have only begun to adapt the methods or to devise new techniques suitable for advertising research. The Rorschach, for example, seems to have little or no application to advertising as a Rorschach. Perhaps there are elements in it which may lead to the development of a technique—no longer the Rorschach test—but a technique useful in gaining new information on advertising motivation.

The administration and interpretation of projective techniques involves so many variables that it is only logical to inspect all conclusions with care. The personality of the psychologist and the conditions under which a test is administered will profoundly affect the responses. Furthermore, to paraphrase a recent public remark of a prominent user of these methods, "The interpretations made by the administrator represent the greatest single variable in the application of projective techniques to marketing and advertising research." We cannot assume that two investigators, working on the same problem will arrive at reasonably similar conclusions, although there are instances where they have done so.

It is noteworthy, that some competent users of projective techniques say all interviewing in connection with these methods must be done by experienced clinical psychologists. We cannot yet confirm or deny that point but, if it is true, it would throw out a great deal of the commercial surveys now being publicized. There is some cause to doubt that the general interviewing level in this type of research has been competent, or even that it has involved the level of training and supervision exercised by leading research companies, working on what has been called quantitative research.

That point raises an interesting question—one which is not academic. It is doubtful that the advertising business could or would support surveys involving projective techniques if trained clinicians were required for all of the interviewing. One practical clinical psychologist recently submitted a proposition to an advertising agency with costs based upon a fee of $25 an hour for the clinical interviewer and $5 an hour for each subject, with an indefinite recommendation that the interviewing consume up to thirty hours per person. This puts the cost per subject up to $900. Such a price would preclude the use of large national samples since the cost would not be found in a commercial advertising research budget.

Still another restraint grows out of the fact that projective techniques are not always required to obtain some of the kinds of information for which they are used. Recently, the Advertising Research Foundation, together with National Analysts, a well-known research company, developed a technique for interviewing on buying habits. The problem was to determine the influence and activities of men and of women in connection with shopping for the home and for the family members. Trial interviews included a wide variety of approaches and techniques, one of which was a Thematic Apperception Test. This device appeared to work effectively in revealing attitudes and emotions of married people while shopping with the spouse. However, it was soon found that the same evidence could be produced more easily through a few simple questions. Therefore, the projective device was abandoned solely because of inefficiency.

It would seem that, up to now, we have been concentrating on cautions in appraising and using projective techniques for advertising research. I do not want to end on that note, for I believe we are going to see much productive use of these methods in the near future. The activities of the Advertising Research Foundation, and many professionally and privately sponsored sessions on this subject, leave no doubt

as to the intensity of interest among advertising research people and the promise which they see of gaining a better understanding of buying motivation.

We must also acknowledge that research companies specializing in projective methods are finding clients nowadays, that advertising agencies keeping up with the trend are doing a profitable business, and that many of the more successful research companies identified with traditional methods are building staff to handle projective techniques. The fact that business is good does not prove that methods are good, but I do not think the people who spend advertising dollars are so blind as to go this far in pursuit of a will-o'-the-wisp.

Perhaps one of the nicest little examples of the effective use of a projective technique is one which represents a by-product rather than a major use. James Vicary, who has made many original contributions to advertising research, has sometimes appended a list of ten word-associations to regular structured questionnaires where field supervision was limited. This simple device must surely challenge the abilities of any interviewer who might be tempted to make himself a multiple respondent. It is doubtful that spurious word responses could long go undetected. While this incidental application is not a strong argument in favor of using projective methods, it does demonstrate their potential significance in revealing the individual personality.

SECTION OF MATHEMATICS AND ENGINEERING

EQUIVALENT AXIAL LOADS IN STABILITY ANALYSIS OF STRUCTURAL METAL BENTS*

By ABRAHAM SLAVIN**

Introduction. This paper presents the use of equivalent axial loads in stability analysis of structural metal planar bents, designed by working load formulas, with members having low values of axial unit stress at the design load. The postulate is applicable to planar bents of structural metals having a well-defined yield point or an offset yield stress. The results verify prior research by the author which indicated the relation of buckling stress to both design stress and yield point or yield stress in planar structural metal truss frames.

The axial stresses are a small part of the total stresses in the members of the bent with resulting low values of the stability parameter of $L/j = L\sqrt{P/EI}$, where L is the length of the member, P the axial load, E the effective modulus of elasticity, and I the least moment of inertia of the member.

With low values of L/j for total unit stresses near the yield point, the application of a unit moment at one of the bent joints to test for stability will not indicate buckling as the series stability factor[1,2] shown in FIGURE 1. For this bent, the author has shown[1] that the effect of axial load coefficients in the analysis for moments and for the corresponding shears may be neglected.

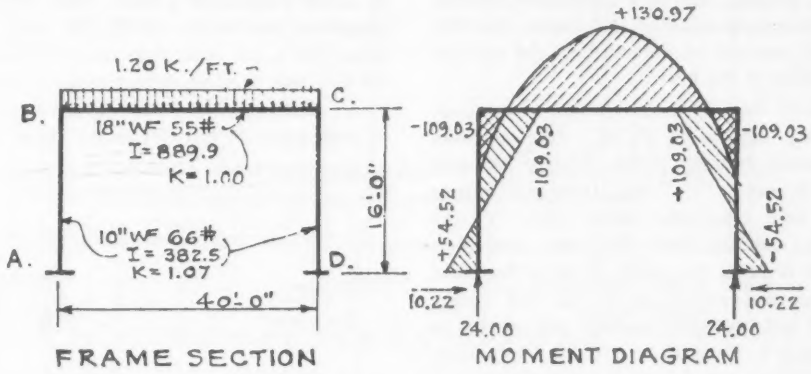

FIGURE 1. Design load.

General. A typical one-story industrial building bent of structural carbon steel, designed under A.I.S.C. specifications, is will be considerably below the critical value of unity. Therefore, the total of the axial unit stress and the bending unit stress in each member of the bent will be multiplied by its respective member area to obtain an equivalent P or axial load. With the equivalent axial loads, the correspond-

*This paper, illustrated with lantern slides, was presented at a meeting of the Section on February 19, 1954.

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TABLE 1. VALUES OF LOADING, STRESS, EQUIVALENT P, E_u, L/j & "r"

LOADING			COLUMN							GIRDER							UNBALANCED MOMENT "r" AT B	
CASE No.	TOTAL LOAD FACTOR	KIPS/FT. ON GIRDER	AXIAL STRESS P.S.I.	BENDING STRESS P.S.I.	f_s. TOTAL STRESS P.S.I.	ΣP IN KIPS	E_u / 1,000	L/j TO AXIAL STRESS	L/j TO ΣP.	AXIAL STRESS P.S.I.	BENDING STRESS P.S.I.	f_s TOTAL STRESS P.S.I.	ΣP IN KIPS	E_u / 1,000	L/j TO AXIAL STRESS	L/j TO ΣP.	TO AXIAL STRESS	TO ΣP.
1.	1.0000	1.20	1,237	17,753	18,990	368.59	29,100	0.278	1.090	631	16,004	16,635	269.32	34,000	0.297	1.525	.0589	.0716
2.	1.5000	1.80	1,855	26,630	28,485	552.89	18,509	0.433	1.698	946	24,006	24,952	403.98	23,700	0.409	2.100	.0593	.0902
3.	1.6667	2.00	2,062	29,588	31,650	614.33	11,902	0.569	2.231	1,052	26,673	27,725	448.88	19,832	0.472	2.423	.0595	.1128
4.	1.7500	2.10	2,165	31,067	33,232	645.03	7,952	0.713	2.796	1,104	28,008	29,112	471.31	17,334	0.517	2.654	.0604	.1476
5.	1.8333	2.20	2,268	32,547	34,815	675.74	3,566	1.090	4.274	1,157	29,341	30,498	493.74	14,502	0.578	2.968	.0622	.5317
6.	1.8500	2.22	2,288	32,843	35,131	681.89	2,639	1.273	4.991	1,168	29,607	30,775	498.24	13,900	0.594	3.050	.0633	4.2233
7.	1.8667	2.24	2,309	33,138	35,447	688.03	1,692	1.597	6.267	1,178	29,875	31,053	502.73	13,206	0.610	3.132	.0658	—
8.	1.8833	2.26	2,330	33,434	35,764	694.17	730	2.443	9.579	1,189	30,140	31,329	507.21	12,650	0.628	3.224	.0761	—

ing values of L/j, for unit stresses in the inelastic range and below the yield point or offset yield stress, resulting from multiple loading, are in a significant amount for the application of the series stability factor method of determining the critical stability of the bent.

Basic data. Tabular values of loading, stress, equivalent P, E_u (the modulus computed on basis of the ultimate load column formula[1,3,4,5,6]), L/j from axial stress and from equivalent axial load, "r" the series stability factor for a unit moment at joint B or C on basis of both the axial stress and equivalent P; for the design load and multiple loading, are shown in TABLE 1. Equivalent P for multiple loading is computed from the total stress in the member on a linear loading ratio.

E_u *and* L/j *versus Load Factor.* As indicated in TABLE 1, the modulus values are less in the column than in the girder. As shown in TABLE 1 and FIGURE 2, the L/j values for axial stress alone are low in both the column and the girder and greater in the girder up to a Load Factor of about 1.40, beyond which the L/j is greater for the column. With the equivalent P, the L/j is greater in the girder up to a Load Factor of about 1.72, beyond which the L/j in the column increases at a more rapid rate than in the girder. For Case No. 8, Load Factor of 1.8833, with a total stress in the column of 35,764 p.s.i., or about 240 p.s.i. less than the yield point, the L/j due to axial stress alone is below known critical values. On the given basis of equivalent P, the L/j in the column is greater than 4.2 for Case No. 5 with a Load

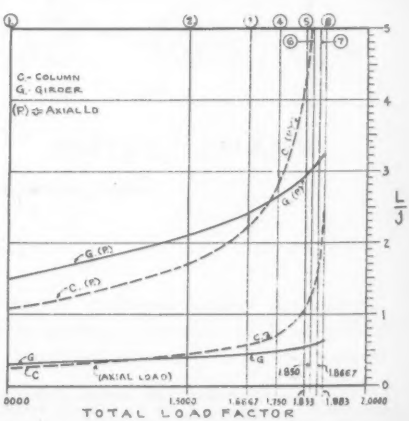

FIGURE 2. L/j versus Load Factor.

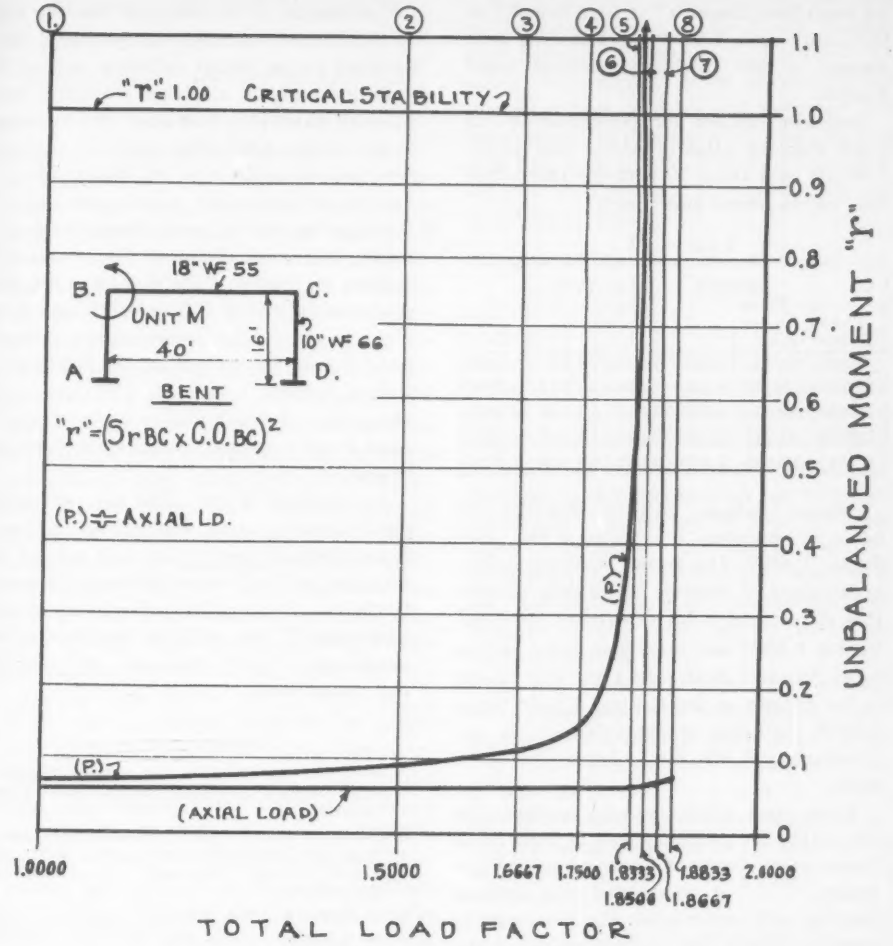

FIGURE 3. "r" versus Load Factor.

Factor of 1.8333, indicating the region of instability.

Stability analysis. As shown in FIGURE 3, the series stability factor or unbalanced moment for a unit moment at either joint *B* or *C*, due to the symmetry of the frame, is equal to:

$$\text{"r"} = [\overline{(S_r\ BC)}\ \overline{(C.O.)}]^2$$

in which S_r is the author's notation for the Stiffness Ratio of member *BC* at joint *B*; *C.O.* is the carry-over factor; in the analysis expression given which represents the moment distribution. There is no return moment from the terminals at *A* and *C*.

Stability factor "r" versus Load Factor. Tabulated values of "r" versus Load Factor for the normal axial stress, and for the equivalent *P*, are shown in TABLE 1. The relation is shown in FIGURE 3. Note that

for axial load alone "r" is less than 0.1 at the Load Factor 1.8833 which loading is in excess of the calculated critical Load Factor.

Pertinent values for equivalent P for Load Factors slightly below and above 1.84 are in TABLE 2, with the taken critical values shown underlined.

TABLE 2

	GIRDER		COLUMN		
Load Factor	Total Stress p.s.i.	L/j	Total Stress p.s.i.	L/j	"r" for P
1.8395	30,000	2.997	34,932	4.500	0.8548
1.8400	30,608	3.000	34,942	4.522	0.9097
1.8408	30,622	3.003	34,957	4.554	0.9872
1.8409	30,623	3.003	34,959	4.560	1.0033
1.8410	30,625	3.004	34,961	4.564	1.0156

Critical values. Critical stability on basis of equivalent P is taken at the Load Factor 1.8409. The series stability factor, or unbalanced moment, is slightly greater than the critical value of unity at the Load Factor 1.8409 and less than unity at the Load Factor 1.8408. The total unit stress in the column at the Critical Load Factor 1.8409, on basis of equivalent P, is approximately 1,000 p.s.i. below the yield point.

From prior comprehensive analyses by the author for planar structural steel truss frameworks, designed by working load formulas,[1,3,4,5,6] it was noted that critical loading will occur when the compression member having the highest value of the parameter L/j is stressed to between 1,000 and 1,500 p.s.i. below the yield point. The computed critical value for the column on basis of equivalent P, is 1,041 p.s.i. below the yield point, which is within the the limiting values from the prior research by the author.

Conclusion. It is indicated that the use of the herein described equivalent P may be used in the series criterion method of stability analysis of planar structural metal bents which have low axial unit stresses at the design load. The practical assumption may be made, that for structural carbon steel frames the yield point may be taken as the critical unit stress. In the example, the critical Stress Factor for the column, on basis of yield point at 36,000 p.s.i., is 34,959/36,000 or 0.971, which is close to unity. The corresponding critical Load Factor for the column is 36,000/18,990 or 1.8957, which is 2.98 per cent above the computed value on basis of equivalent P and is considered within a practical tolerance.

An analysis of the same bent of structural aluminum alloy, which does not have a well-defined yield point, and subject to multiple loading,[1] also indicated the practicability of using the author's suggested equivalent P for stability analysis under conditions of low axial unit stresses at the design load.

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RESEARCH ON CONSTRUCTION MACHINERY AND CONSTRUCTION MANAGEMENT, HERE AND ABROAD*

By GEORGE GARBOTZ**

Historical Review

An organized and comprehensive treatment of what had previously been chaotic and scattered material on the subject of construction machinery was undertaken for the first time in the Handbook of Engineering Sciences, printed in 1883;[1] publications on the problems of construction management first appeared toward the end of the twenties.[2] Large-scale construction of roads started in 1926 with the introduction of concrete road pavements and the mechanization of such construction.[3] This type of construction flourished with the great increase in the use of automobiles and the construction of superhighways after 1933.

Published work on the scientific treatment of construction and management problems through applied research began with the predominant use of machinery on construction sites and is only 20 years old. It is limited to a series of studies on scientific equipment conducted by the Bureau of Public Roads in Washington, D.C.; to the published results of several investigations made by the Road Research Laboratory in Harmondsworth, England into mixers, finishers, and rollers; to studies of mixers published in Sweden and Denmark; and to 20 works on research published by the author's Institute of Construction Machinery and Construction Management until 1950 at the Technical University in Berlin and, since 1950, in Aachen where five more works on the subject were in preparation in 1953.

Not all of these investigations have been of the same practical importance. In general, however, the results have yielded valuable suggestions, have stimulated the thought of both producer and consumer, and have helped to develop a rationale of construction management. This fact becomes obvious on investigating the development of concrete mixers.

Equipment for Concrete Construction

Results of tests on the influence of mixing time on the strength of concrete were published in Canada by Abrams[4] in 1918; in Germany by Kleinlogel[5] in 1926; and in the United States by Harrison[6] in 1926. But systematic tests on as many as 30 mixers of various types operating intermittently and continuously, on nontilting and pugmill mixers, and on both small and large types (150 to 500 liters) were not made until the early twenties, when Graf and Garbotz[7] conducted such tests. The research of these investigators yielded the following results:

(1) Dry premixing gives *low strength* only. Water should be added *before* charging the mixer with aggregates (FIGURE 1).

(2) An extension of the mixing time over 60 seconds for lean, and over 90 seconds for rich concrete mixtures is uneconomical.

(3) It must be possible to measure the water quantitatively with an accuracy of

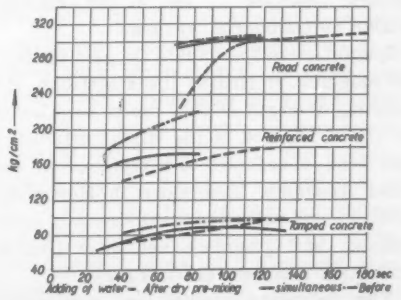

FIGURE 1. Adding of water and strength of concrete.

*In the absence of Doctor Garbotz this paper, illustrated with lantern slides, was delivered by Doctor Wenceslas Sivel, Director of Research, Parsons, Brinckerhoff, Hall & Macdonald, Consulting Engineers, New York, at a meeting of the Section on October 23, 1953.

**Professor at the Technical University, Aachen, Germany and Director, Research Institute, Construction Machinery and Construction Management, Aachen, Germany.

±3 per cent and add it within 15 to 20 seconds.

(4) The quality of the mixture depends on the velocity of the mixing parts of the machine and the water volume of the mixing tank.

These trial-test investigations were later completed in 1936-1938 by observations made on eight Autobahn bridge-mixers having a capacity of 1000 to 1500 liters.[8] Considering these studies from the viewpoint of management problems, the following additional results were obtained:

(1) For stiff and rich concrete mixtures for superhighway construction, the mixing time of nontilting mixers should not be below 90 seconds, and not below 60 seconds for the pugmill mixer.

(2) Under the condition mentioned in paragraph 1, the number of cycles per hour of the nontilting mixer may be assumed to be about 20 to 22, and 32 to 35 for the pugmill mixer, with an unavoidable delay factor of 20 per cent.

In fact, there are considerable undeveloped potentials of production power, which can be seen from the comparison of German[9] and American equipment production studies of the same period.

Finally, there have been performed in recent times some comparative and some single studies on various types of mixers, such as tilting mixers, pugmill mixers, and continuously working mixers, in England,[10a] Sweden, Denmark,[10b] and at the Institute headed by Garbotz at Aachen.

Special attention should be paid to British research. In British investigations, the crushing strength of test tubes as a basis for the evaluation of the mixing efficiency has been abandoned and replaced at the Road Research Laboratory in Harmondsworth by the method of water-cement, sand-cement, and gravel-cement ratio (factor) from 5 kg. samples. (FIGURE 2)

The following results are particularly interesting:

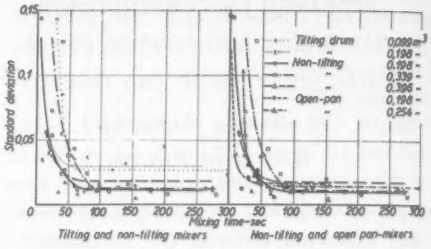

FIGURE 2. Mixing time and water-cement ratio.

(1) The mixing process is finished within 60 to 90 seconds.

(2) Material and mixing ratio do not influence the mixing time.

(3) The pugmill allows shorter mixing time, but does not produce uniform concrete.

(4) Tilting mixers produce a concrete which varies greatly in strength and workability.

The mixing process was clarified by all these investigations to such an extent that at the last meeting of the German Concrete Association, new standard specifications for grading concrete mixers were established.[11]

General research has taken a similar pattern in the development of concrete road finishers. Such research began in 1936 with experiments which were performed by the Institute headed by the author, in conjunction with Graf and Eitel, at a construction site on a section of the Autobahn.[12] The object of these investigations was to determine whether the finishers in

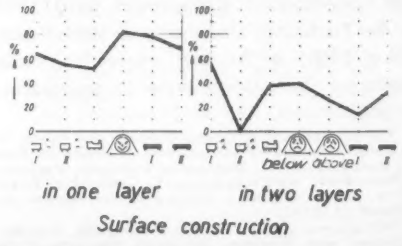

FIGURE 3. Utilization of available working time in concrete road construction.

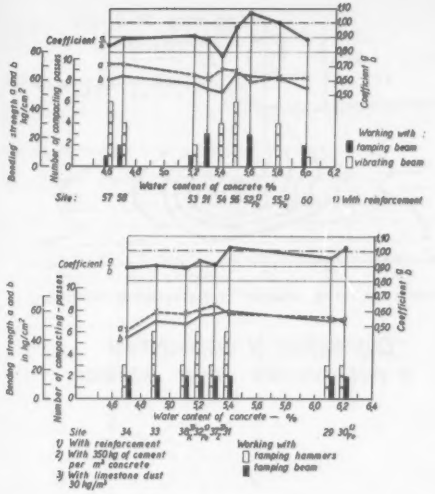

FIGURE 4. Compacting effect at surface constructions in one layer.

current use could compact concrete slabs of 20 to 25 cm. thickness in *one* layer. (FIGURE 3) As a criterion, the ratio of the flexural strength on the *top* and *bottom* of an exposed concrete slab 1.3 meters by 1.4 meters was used. The essential results were as follows:

(1) The compaction of one layer of this thickness is easily possible with the "Dingler" tamping-hammer and with the Vögele vibration-finisher (FIGURE 4).

(2) For the establishment of a smooth surface on the surface coat, the finishers are sensitive to the concrete stiffness. For the compaction and the establishment of a smooth surface on the surface coat three to five passes are necessary.

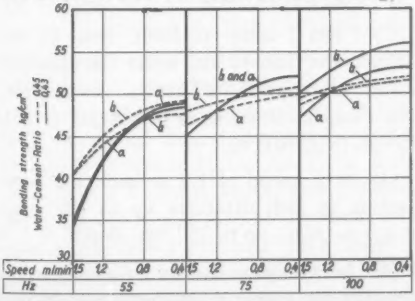

FIGURE 5. Influence of frequency and water-cement ratio on strength.

At the end of the thirties, other experiments were started at the Research Institute to determine if, and under what conditions, the compaction and the establishment of a smooth top surface-coat are practicable by working in one pass (*i.e.*, working continuously) with the usual finishers on a 22 cm.-slab thickness.[13]

The following results were obtained:

(1) With the vibration-finisher, a continuous working process is possible with a speed of about 0.4 to 0.8 m./min., the water-cement ratio being 0.43, using frequency of 70 Hz. = 4200 v.p.m. for the compaction, the double amplitude 2a being smaller than 0.3 mm., and using a beam of 30 cm. width. For the compacting of the top-surface layer, a vibrating beam of 35 Hz. = 2100 v.p.m.; the double amplitude 2a smaller than 0.2 mm. and a width of the beam greater than 30 cm. should be used (FIGURE 5).

With increasing frequencies, the depth effects, the flexural strength, and the workability of the concrete increases. The amplitude should be uniform over the entire length of the beam, and the number of vibrations should be more than 2000 v.p.m. per running meter.

(2) With tamping beam-finishers, a continuous finishing system is not possible.

(3) For wet concrete having a water-cement ratio of 0.52, an oscillating screed is sufficient. Operating speeds up to 2 m./min. were obtainable if the beam was vibrated with a frequency of 35 Hz.=2100 v.p.m. The possible amount of increase in production can be shown by the average operating speed, which was assessed at 0.13 to 0.33 m./min. in the above-mentioned production studies; whereas in the United States a speed of 0.6 m./min. was obtained.

In 1949 and 1951 the Road Research Laboratory at Hamondsworth, England, in-

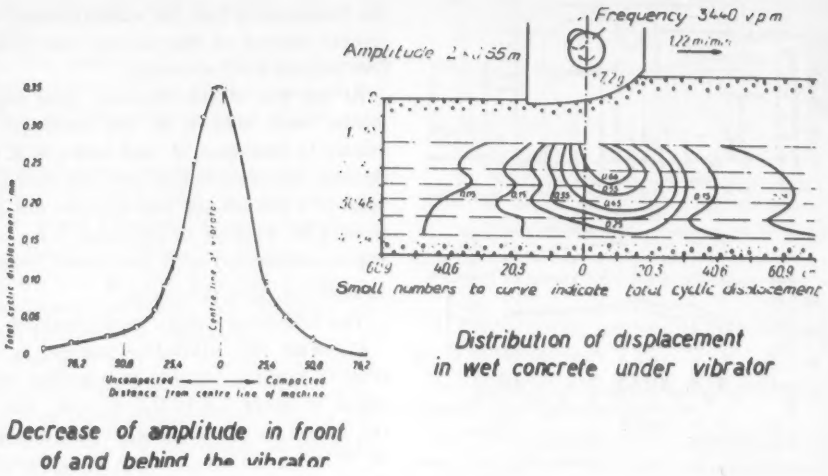
Frequency 3440 v.p.m
Amplitude
Uncompacted
Compacted
Distance from centre line of machine
Small numbers to curve indicate total cyclic displacement
Decrease of amplitude in front of and behind the vibrator
Distribution of displacement in wet concrete under vibrator

FIGURE 6. Displacement of freshly placed concrete.

vestigated the fundamental relations on surface vibration of concrete slabs,[14] and the results obtained constitute an extremely valuable contribution to the research of finishers.

The results of these investigations were never more eagerly awaited than in the years after the war. The question which continually arose pertaining to the construction of runways for heavy airplanes was whether slabs of a thickness up to 65 cm. could still be compacted with the surface vibrator. This question, in the meantime, has been answered positively by site-tests made by the author in cooperation with Hummel, on an A.B.G. finisher and by shop experiments conducted by Vögele in his factory in Mannheim. The British studies are of the greatest interest to experts for their unusually precise measuring methods.

In these tests it was possible, by using a finisher especially designed and constructed for these experiments to change to a great extent the operation speed, the frequency, the form of vibration waves, the amplitude, the weight of the beam, and the shape of the shoe; as well as the mixing ratio and workability of the concrete, the thickness of the slab, and the subgrade.

Originally, the degree of compaction was measured by the density of cores of 150 mm. diameter, as was done in the most recent German experiments. Later on, this method was replaced by measuring the degree of compaction by the rate of transmission of gamma rays emitted from a radioactive element through the concrete. The most important results, some of which have been mentioned previously, may be summarized as follows:

(1) From 1 meter in front, from 80 cm. behind, and from 40 cm. under the vibration beam, vibrations are hardly discernible. The compaction extends to a depth of only 30 cm. (FIGURE 6).

(2) At a speed of 1.2 m./min. the compaction is still effective up to 100 mm.; at 0.6 m./min., up to 250 mm. depth.

(3) Acceleration has no influence on the compaction; higher frequencies result in a better surface quality.

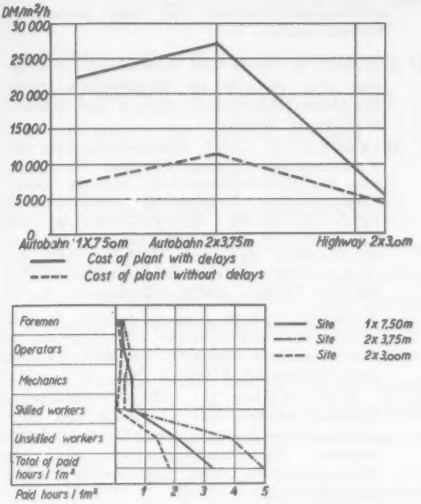

FIGURE 7. Cost research in concrete road construction.

(4) Higher amplitudes do not improve compaction but do improve the depth effect.

(5) For compaction purposes, the finisher should operate with high amplitudes and low frequencies. To attain density of the surface course, it should operate conversely.

Finally, two more studies from the field of construction plants and machinery for concrete road pavements are of interest. V. Glyszcinsky[15] has made comparative studies on the production and economy of surface pavement at construction sites having a width of 7.5 meters or two lanes of 3 meters width each. In spite of unfavorable local conditions which influenced the comparability, it was ascertained that the specific costs of plant equipment and the wage expenses reach their minimum if the surface concrete is placed by the American method in strips of 3 meters width, and if delivery of materials to the site is by truck. Postwar construction methods have been influenced by these findings. The second study has been made in the Institute headed by Graf and concerns the testing of hand-guided vibration and tamping-compaction machines, as constructed by a few companies in Western Germany (Flottmann, Wacker, Losenhausen, Frisch, and others). The results of these studies confirm the fact that these small machines are able to compact concrete slabs of 20 cm. thickness with a penetration coefficient of $e_{15}=2-6$ (according to German standards) in one to three passes, or 1.5 min. vibration period per square meter.[16] However, the use of such slabs is restricted to subbase courses only, as a level plane surface cannot be obtained with such equipment.

If we review the research performed on highway engineering, we learn that much has been done in the field of material and construction methods, but that as regards research on the actual production process, sufficient systematic research has not yet been undertaken.

Only as actual problems have arisen has money for research been made available by interested firms and clients requesting specific investigations. The above-mentioned American production tests may be the only exception to this rule. It will require the understanding and the cooperation of all interested groups throughout the world to overcome the existing deficiencies.

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SECTION OF ANTHROPOLOGY

COMMENT ON FIELD TECHNIQUES IN ETHNOGRAPHY, ILLUSTRATED BY A SURVEY OF AMAMI OSHIMA, RYUKYU ISLANDS*

By DOUGLAS G. HARING**

A six months' ethnographic survey of the island of Amami in the northern Ryukyu Islands, undertaken in 1951–1952 for the Pacific Science Board of the National Research Council, under contract with the United States Army, occasions these remarks about field techniques. A systematic discussion of ethnographic procedures is beyond my purpose.

Ironically, I have frequently stressed the unreliability of data obtained in brief field trips; now I offer only a scant six months on Amami, minus time lost by typhoons, Amamian deluges, and obligations incurred by playing host to certain microorganisms.

The Amami folk naively subscribed to this dim view of short-term research; they tempered their farewells with a benevolent *caveat:* "Remember, you have been here only six months; you have just begun to know Amami!" I still insist that no one really knows an alien culture short of many years' residence.

Amami, focus of Amami Archipelago, ranks next in size to Okinawa among the Ryukyu Islands. This triangular island is 36 miles long and 18 miles across the base. Rugged mountains and deep fjords offer thrilling scenery and a meager subsistence for the 110,000 people who eke out a precarious livelihood from tiny valleys and crudely terraced mountain sides. Naze, the capital, contains about 34,000 people; the next largest town has perhaps 10,000. The remainder dwell in tiny hamlets in isolated valleys and sheltered bays—often accessible only from the sea.

Amamians have long been part of Japanese civilization. They also have much in common with the small peripheral societies. This dual orientation I shall sketch briefly. Thanks to Japanese public schools, the people know standard Japanese and are literate. They are proud of more than 800 years of recorded history. For three and a half centuries they have been integrated in the Japanese political orbit, although until about 1875 their status was that of serfs. They read both locally published newspapers and reading matter from Japan. Some households have radio and, in the larger places, news and advertising are dispensed over public address systems. Urban Amamians are accustomed to movies and dance halls, bingo games, cockfights, athletic meets, and to well-stocked shops. The endless rains and steep mountains afford hydroelectricity for the cities and for many of the hamlets. Ocean-going ships call regularly at Naze, and motor sampans offer cheap interisland transportation. Roman Catholic, Buddhist, and Shinto missionaries carry their doctrines to most of the hamlets. The intelligentsia include highly educated individuals who wield considerable influence. Sometimes a peasant will interrupt his toil to tell with glowing eyes of his son in a university in Japan. Agricultural extension services have introduced scientific agriculture and stock-breeding. All these things are *modan*—the Japanese version of "modern."

Amami's simpler past, however, still lingers. Sixteen local dialects, unintelligible to the average Japanese, survive in mountain-girt hamlets and isolated islands. Extreme poverty isolates the peasants, whose world is circumscribed by the prac-

*This paper, illustrated with kodachrome slides and motion pictures was presented at a meeting of the Section on February 22, 1954.

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tical question, "Do we eat rice today, or do we wash down cycad mush with the water used to wash yesterday's rice?" Shamanism, magic, and witchcraft are ubiquitous; city people ridicule superstition, but fear to venture into a strange village, lest the very water be charged with a fatal spell by the hissed imprecations of a witch. An ancient cult of living female deities—the *Noro*—survives in the countryside. Beliefs in goblins, in tree spirits, in fearsome half-human, half-animal offspring of unholy revels of practitioners of black magic, in sinister powers acquired by ghastly manipulations of blood drawn from the snipped-off tongue of a starving dog, in the efficacy of moxa cautery and acupuncture, in the benign guardianship maintained over a man by the spirit of his elder sister, in the mischief of ghosts and sprites—such beliefs are no monopoly of villagers. Buddhism has made but little headway, perhaps because Buddhist priests have been male, for Amami tradition accords primacy to women in religion, and perhaps because the Amamian love of pork—a trait related to South Sea influences—conflicts with Buddhist taboos on meat. Family patterns and the status of women avowedly conform to Confucian and feudal-Japanese models. The rationalizations indeed are Confucian, but the real freedom of women, the investment of romantic love with glamour, and the lavishing of affection upon both consanguine and affinal kindred, suggest that the human warmth of early Japan and the South Seas has outlasted what Sir George Sansom once called "the Confucian malady."[1] Under a hand-to-mouth economy very little money passes through the hands of farmers and fisherfolk. Even urban industries, such as the weaving of *tsumugi*—an exquisite *ikat*-dyed silk textile—continue as handicrafts refined by scientific research. The distinctive aspects of Amamian personality survive even after migration to Japan; the expatriates foregather in Japanese cities and nostalgically review the good old days back home. Thousands of young Amamian laborers work on United States Army projects on Okinawa; what is happening to them is anyone's guess.

Against the background of modern and not-so-modern Amami the ethnographer seeks to describe and understand. The following discussion of field procedure is frankly in personal terms, since a depersonalized ethnographic report closes an important avenue to appraisal of the findings. An ethnographer is a human being precariously adjusted to human beings culturally different from himself. Only as the pattern of that personal adjustment is revealed can the trustworthiness of his account be estimated.

Conditions were unusually favorable for research. Everywhere I met with kindness and hospitality. Two suspicions had to be allayed; first, that I was an American counter-intelligence agent; second, that I might be working for the defunct secret police of Japan. Amamians were upset profoundly over the new United States hegemony divorcing them politically from Japan proper. The strange military regime that lumped Amami with Okinawa and interposed an incomprehensible boundary between Amami and Japan had disarranged the patterns of social expectation. Across that boundary, in Japan, were friends, relatives, fiancées and in-laws, children in college, business connections, pensions, old-age insurance, veterans' benefits, and sentiments. Geographically a close neighbor, Okinawa long had been viewed with rural resentment toward the city slicker and, except for its northern tip, was a place where no relatives lived and whence no conceivable aid could come.

Amamians manifested an ambivalent combination of fear lest they unwittingly drag me into politics, and deep urgency lest I fail to perceive their desperate longing to rejoin Japan. I began to wonder how the

people of the State of Maine might respond if suddenly they were cut off from the United States and incorporated into French Canada. At least the Amamians were grateful for American aid and friendship; they worried lest agitation for reversion to Japan be construed as hostility toward the United States. This tense feeling created a situation which could have biased the research: it was apparent, however, that any honest survey would list as an outstanding fact the universal desire to rejoin Japan. This political hope motivated much of the ready cooperation with the ethnographer.

Desire to win backing for reversion to Japan, however, was not the sole factor in acceptance of the ethnographer. I am convinced that even now, with reversion to Japan accomplished, an anthropologist would be equally welcome. For the people literally adopted the study; they wanted to be studied, they were pathetically eager to have their customs recorded, to be noticed in the outer world. The visiting ethnographer symbolized their longings for a breach in the isolation. Apart from objective findings, I should have been less than human had I failed to respond and accept this emotional load, knowing full well that no man can cope with the hopes and dreams of a whole people.

The inherent assets of the situation transcended this emotional complication. Japan's senior scientist, the ethnographer Doctor Kunio Yanagita, who is revered on Amami for his studies of Amamian culture, had generously written letters on my behalf. The two daily newspapers reported the project carefully, and made it clear that this was the sort of research that anthropologists conduct in their own countries, that it involved no assumption of inferiority of the people studied; and they continued to provide an open channel to the people. Another asset was the prestige of science among Amamians; even the peasants are convinced that the future belongs to science. At Naze a pathetically ill-housed museum stocked with books, a collection of shells and archaeological artifacts, was crowded with eager students. The assistant curator was a brilliant young self-taught anthropologist, and the trustees of the Museum generously placed him at my disposal for the duration. In addition, several capable local antiquarians stood ready to discuss lore and custom, to obtain singers for the tape recorder, or to introduce me to some village mayor.

Knowing the workings of gossip and curiosity respecting a foreign visitor, I deliberately planted correct information about myself in the fountainhead of gossip — a *geisha* house. Dropping in for lunch, when the girls were not occupied professionally, I answered their questions about my personal history, family and grandchildren, job, prospects, and all normal topics for gossip. This ruse worked so well that later on all this information returned to me—plus normal accretions—from the familiar spirit of a shaman in trance as he disclosed my past sins and future fortunes. Obviously the spirit was not above consorting with *geisha* girls.

I employed no "informants" in the conventional sense; I did hire two young translators—the one to render into English for me the local newspapers and other periodicals, the other to act as interpreter. The interpreter's translations of interviews insured my hearing everything twice and gained time for note-taking. When unusually heavy downpours curtailed our activities we held informal sessions on the aims and results of anthropology—followed usually by enthusiastic quests for new data.

Several associations cooperated voluntarily. The Federation of Women's Clubs sponsored questionnaires on marriage, kinship and family life, criticized and amended my schedules, mailed them to local clubs, and coaxed back the late returns. When a local camera club was organized, I joined

the Colonel in command of the United States Civil Administration unit in offering prizes for a photographic contest that stressed handicrafts, customs, and scenery. The Physicians' Association volunteered active cooperation, and I regret deeply that limited time prevented me from taking advantage of that offer. Government offices, especially the Statistical Office, provided information on request; despite lapses during war years and defects in reporting, their records were invaluable. At the Boys' High School many of the boys kept diaries for two months for my use; at the Girls' High about 20 girls persuaded aged persons to dictate autobiographies, and these documents shed light on history, customs, and—dimly—on psychology. Teachers, both urban and rural, were willing, perceptive, informed aids in research. Studies already made locally provided much help: for example, hard-pressed by rising expenses, the teachers had just published a detailed study of family budgets and costs of living.

In order to analyze printed media of communication, all locally published printed materials were assembled as of a set day each month, when they were filed and translated. The leading bookstores regularly provided lists, including quantities, of books and periodicals imported from Japan proper.

Daily research and participant activities were recorded in a log that ultimately covered 346 closely typed pages. Among the significant participant activities, the fortunate accident that forced me to make a speech in Japanese at an educational conference went far to settle the question of popular acceptance of the ethnographer; thereafter, wherever I went, someone who had heard the speech would greet me as an old friend, and immediately the people would answer questions, pose for pictures, and surfeit me with cooperation. Even the secrets of the *Noro* cult eventually began to drift to my ears.[2]

The Wenner-Gren Foundation generously supplemented my equipment with a 16 mm. movie camera, a tape recorder, and moderate funds for supplies. These mechanical recording devices attracted the interest of the Amamians: professional artists volunteered to record songs, friends sought out old persons expert in obsolescent dialects, and villages begged to provide folk-dances (which were out of season) for the movies. Movies are invaluable to record technology, dances, rituals, and any other activity that conforms to a set pattern. The intricate dyeing and weaving of Oshima *tsumugi*—perhaps the most precise and exacting handicraft known—yielded to cinema recording in spite of my ignorance of textiles and weaving.

Ethnographic movies are records, not spectacles. To the extent that conditions are rearranged to suit the camera and rehearsals are held, bias may be introduced so subtly that the ethnographer does not detect the shift in emphasis. Photographing without prior rehearsal consumes extra film and may yield indifferent entertainment, but it can obtain a reliable record; and an accurate record with minimum disturbance of customary routine is a defensible goal when the ethnographer has but one movie camera and no technical assistant, and deals with a native situation unamenable to control.

Fundamental in study of a literate people is the question of imparting the results of the research to the people studied—a crucial problem of ethnographic research that is unknown in the physical and biological sciences. The subjects of social research are self-naming, self-explaining, and self-categorizing. Still more important, they want to know the results, and unlike laboratory rats, they regard the findings with emotion. In addition to keeping the public informed concerning my activities, I invited leading citizens to view the pictures and listen to records just before I left Amami. So they knew exactly what I carried away,

and their comments opened new insights, verified information, and corrected blunders. As far as reports of research can be written before leaving the field, they should be read in the native language to native critics. This procedure was followed as regards my preliminary reports, except for recommendations made to the United States Army. These critics had been picked to include some of the best-educated and most thoughtful islanders, and they corrected numerous mistakes in addition to verifying points over which I was unsure. Later, I showed pictures to a group of Amamians in Tokyo, who were enraptured with glimpses of home, and volunteered independent confirmation of many facts. Also in Tokyo, Doctor Yanagita kindly assembled a group of Japanese ethnographers and linguists to whom I presented a summary of the findings; their questions and comments were invaluable.

Every philosopher knows that choice and formulation of a question define the range of the answers. I think that introduction of technically refined questionnaires or public opinion schedules into a non-Euro-American milieu may involve serious bias from the ethnographer's unconscious acceptance of the logic of his own culture. When the Amami Federation of Women's Clubs revised my schedules, I took the hint: Why not let the people themselves make out the questions? I made several studies on this principle; unfortunately a box that contained all the relevant schedules, replies, and translations—together with translations of the native press and all data of kinship and associations—was lost in transit to the United States.

The first use of native ingenuity was in a survey of associations in Naze. I employed an intelligent and influential man to secure descriptive data on all discoverable associations. A member of the Assembly, he already knew many groups, and political aspirations whetted his appetite for further contacts. He formulated questions and discovered categories appropriate to the Amamian concept of an association—a concept foreign to American habits of thinking.

Another study dealt with mate selection and family customs. The investigator in charge of this study was a respected teacher of Home Economics in the Girls' High School; in the homes of hundreds of former students she was a more than ordinarily welcome guest. I offered a list of questions and suggested that she revise it. She produced an interview schedule different from anything I could have devised, and in some respects it was highly satisfactory. After that study she proposed an idea of her own: she had been teaching facts about birth and infant care, and wanted to learn the facts as the midwives had discovered them. Would I help her plan an interview schedule for midwives? Promptly I offered to pay the costs of the study if she would share the results. The data so obtained are no model of interview technique, but some questions and answers are most significant. Happily this study was not lost.

My general conclusions may be stated briefly. Unusual opportunities combine with special problems in the study of a literate people whose numbers are small enough to render conspicuous an ethnographer in their midst. Ethical questions implicit in all ethnographic research come to focus when subjects are sure to read whatever appears in print about themselves. On occasion, such a people may tend to center hopes and dreams in the ethnographer as a link with the outside world, and thus to complicate his scientific and ethical problems. This involvement is a facet of the durable question of whether the rich cultural repertory of a literate people can be surveyed in the way that has yielded such valuable studies of peripheral tribes. Certainly Malinowski's brilliant portrait of the ethnographer at work[3] scarcely pertains at all to the study of a literate, relatively populous society—

and increasingly, ethnographers will deal with this latter type of situation as literacy becomes more widely diffused.

A people who read and discuss what they read should be taken into the ethnographer's confidence. He will respect confidential information, but otherwise he has everything to gain by the sort of publicity that invites verification. If individuals suspect that the ethnographer is misinformed or purblind on any topic, someone will undertake to set him right. Furthermore, native minds should be enlisted to aid in various aspects of planning and investigation, in the hope of escaping from the ethnographer's own cultural limitations. Native critics can contribute richly to the verification of findings. On the other hand, an ethnographer brings to the culture insights that may offer the natives new understanding of their own ways of living; their support of the research is enhanced when an aspect of their culture of which they have been but dimly aware is clarified to their surprised satisfaction. Some of Reik's ideas[4] are interesting in this connection.

No amount of sympathy with native culture justifies distortion of fact. One can endeavor to hold to a mature and inclusive objectivity that recognizes the emotional responses involved. If these remarks stress the emotional aspects of my life in Amami, it is because the situation forced upon me a deeper understanding of what I once wrote about the late Professor Thurnwald: "... if they (ethnographers) study exotic peoples, they remain under an ethical obligation to consider ways in which their findings may aid those peoples to survive and attain the goals they seek. Professor Richard Thurnwald ... never subscribed to the doctrine that the fate of those whom he studied did not concern him ... the ethnographer cannot forget that he is dealing with people whose very existence may be threatened by the civilization that he represents."[5]

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DIVISION OF MYCOLOGY

EXPERIMENTAL MICROSPORUM LANOSUM INFECTION IN DOGS, CATS, AND RABBITS. I. OBSERVATIONS ON THE COURSE OF THE PRIMARY INFECTION*

By FREDERICK REISS, LEONA CAROLINE, AND LOUISE LEONARD**

The constant search for new antimycotic remedies indicates that the therapy of most superficial fungus infections is still far from being satisfactory. Furthermore it is well known that a great discrepancy exists between the *in vitro* effect of fungicidal medicaments and the therapeutic efficacy *in vivo*. Among the number of chemicals synthesized in recent years some exert a fungistatic effect in culture media in a dilution up to 10^{-6}. Such fungistatic assays have been disappointing because of the poor therapeutic response. Most mycologists realize, therefore, that the antifungal properties of a drug cannot be evaluated on the basis of growth inhibition on agar plates. In view of the existing discrepancy between the *in vitro* tests and *in vivo* effect, various other methods and approaches which could be utilized to a greater degree, in the evaluation of antifungal remedies, have been sought.

Present and past investigations. The present study was undertaken to investigate the course of an experimental *Microsporum lanosum* infection in dogs, cats, and rabbits, primarily for the purpose of screening antimycotic remedies. The dog and cat appear particularly suitable for this infection because *M. lanosum* infection is a naturally occurring disease in these animals. It added interest to make systematic observations on the clinical course of *M. lanosum*, previous observers having made only scanty and contradictory references in this respect. The literature is not only replete with discrepancies regarding the course of the disease in animals, but there are also contradictory statements regarding the cure of the disease in animals. Sabouraud, Suis, and Suffran[1] reported the naturally acquired infection in 15 dogs of different species. The age of the dogs ranged from two months to five years. These authors described the various stages of the disease, and stated that the duration of the disease depends, seemingly, on the extent of the lesions; a cure was achieved with tincture of iodine in three to four months. The authors observed that healthy dogs kept in the cages with infected dogs did not contract the disease easily. From this fact they came to the conclusion that some "extrinsic factor" is involved in the production of the infection. Several authors Perpignano,[2] Hagan and Bruner,[3] and Murrell[4] refer to ringworm infections in dogs but none of these investigators gave exact data on the duration of the infection or a description of the course of the disease. Observations are also rather scanty in relation to experimental tinea in cats. Davidson and Gregory[5] infected experimentally one young black cat with *M. lanosum* infected hair. The infection apparently healed in five weeks, but fluorescent hairs and positive cultures persisted for three months. These authors also reported on an *M. lanosum* infection transmitted to a kitten from a boy. The infection in the kitten lasted only six weeks. According to Bruhns and Alexander[6] experimental *M. felineum* infection has an incubation period of nine days. After this time there is crusting and thickening of the infected skin followed by

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loss of hair. Lombardo[7] has also found an incubation period of from seven to nine days in guinea pigs. The acme of the disease is reached on the seventeenth day and the healing takes place in 50 days. In an investigation on rabbits the incubation time was five to seven days and the healing occurred in one month. De Lamater and Benham[8] produced a few infections in cats with *M. felineum (lanosum),* which caused a mild inflammatory reaction. The lesions persisted for about 100 days and the fungi could be demonstrated as late as the eighty-fifth day.

Materials used for inoculation. (A) *M. lanosum* culture recently isolated from a five-year-old dog presenting a natural infection. This culture was isolated and maintained on 4 per cent glucose, 1 per cent neopeptone agar (Difco). For inoculation of the infected animals fresh 10-day-old cultures on 4 per cent glucose, 1 per cent neopeptone agar (Difco) were used. Stock cultures were maintained under mineral oil. (B) *M. lanosum* infected dog hairs, markedly fluorescent, from a mongrel puppy which we infected with the above culture.

Method of inoculation. The selected areas on the flanks of the animals were clipped with an electric shaver and then abraded with sandpaper. An emulsion of the culture in sterile vaseline was rubbed into the abraded area. All animals received this type of inoculation. In addition, the dogs and cats were inoculated on the contralateral flank with fluorescent, *M. lanosum* infected dog hairs. The infected dog hairs were gently rubbed over the entire abraded area and then sterile vaseline was applied to keep the infected hairs in contact with the inoculated area. The inoculated area was covered with sterile gauze and kept in place with adhesive tape.

Observations

The following data may not be sufficient to draw valid conclusions; however, observations have been made on the course of experimental *M. lanosum* infection which may be of certain inherent interest. As far as we could ascertain, no previous investigators have ever recorded the course of the disease in rabbits, newborn rabbits, cats, kittens, puppies, and dogs in association with fluorescence and mycological studies. The most characteristic features of each animal species are the following:

Rabbits (nine weeks old at the time of the infection). These rabbits showed a rather intense inflammatory response to the infection. The duration of the symptoms (59, 94,94,108,114 and 115 days) as well as the recovery of fungi by cultures (110,29,69,70, 114,48 days) showed great individual variation. The duration of alopecia was not considered as a distinguishing clinical feature of the infection in rabbits since Rony[9] indicated that there are cyclic variations in hair growth. The shifting of the fluorescence from the entire hair shaft to the tip of the hair is a peculiar feature of the infection in the rabbit. This unusual localization of fluorescence corresponds actually to the location of the spores, and in such instances the spores are lodged 2 mm. proximal to the apex of the hair. Duration of fluorescence ranged from 21 to 112 days in the various individuals.

Newborn Rabbits (inoculated 24 hours and 10 days after birth). These rabbits showed a hardly perceptible inflammatory reaction and moderate crusting. Although the appearance and the duration of fluorescence showed some variation (84,97,111, 119 days), the persistence of clinical signs (alopecia and scaliness) and the recovery of the fungus (100,106,111,119 days) were fairly common. The gradual shifting of fluorescence to the tip of the hair was also observed in these animals.

Dogs (Chihuahua, 77 days; Boston Bull terriers, 69 days; and mongrel puppies, 71 days old at the time of the inoculation). The duration of mean fluorescence (54 days)

was about the same in all the dogs. Similarly, the response was greater with the culture inoculation than with the disease produced with infected hair in all these animals. This observation is in contrast to that of Kligman[10] who observed a better response to infected hair in humans. Although alopecia lasted in these animals from 105 to 181 days, this symptom is questionable as a clinical sign of an infection because it may be a resting stage of the cyclic hair growth. On the other hand, it is noteworthy that positive cultures have been obtained from scales from 8 to 57 days after the disappearance of fluorescence and the last positive cultures were obtained for 57 days. This indicates that, in these animals, as in humans, the disappearance of the infection can be judged only after several negative cultures have been obtained. A spontaneous infection developed in one Boston Bull terrier and in all the mongrel dogs. In these animals the infection was of a relatively mild inflammatory nature, which indicates that this fungus is adapted to the host. In one of the mongrels, onchomycosis of one nail developed. This condition lasted only 32 days. Apparently fungi are not as deeply anchored as in human nails.

Cats (Alley kittens, 57 days; Siamese kitten, 60 days; and adult Siamese cats 1 year + at the time of the inoculation). All alley kittens developed widespread spontaneous infection, accompanied by moderate erythema and partial alopecia. The perioral area showed intense fluorescence in all animals but none of the tactile hair became infected. This may be owing to richer blood supply than is present in the coat hair. According to Melaragno and Montagna[11] the tactile hair is surrounded by elaborate blood sinuses. These sinuses are supplied with arterial blood, which percolates through the cavernous meshes, and the phagocytic cells remove any debris present. The latter function may explain the uniform absence of fungus infections in tactile hairs. In the female Siamese kitten the infection was rather progressive, involving large areas having a more marked alopecia. In this animal the perioral area was heavily infected but the tactile hair also remained free from the infection. A peculiar muddy yellow discoloration was noticed under the Wood's light in the regrowing hair. With Hotchkiss McManus stain a diffuse reddish material could be demonstrated, but no evidence of formed fungal elements could be seen. In the adult male Siamese cat a rather widespread infection was observed despite the sexual maturity of the animal. Autoinoculated areas also developed, but were not as widespread as in the immature kitten. The period of fluorescence correlated exactly with the recovery of positive cultures (68 days). The adult female Siamese cat did not present noteworthy clinical features except the "bluish fluorescence" of the hair tips. The hair in the inoculated site showed a brownish coloration, as in the other Siamese cats. A pigmentary change at the site of the inoculation has also been observed by De Lamater and Benham[8] in *T. gypseum* infected guinea pigs. In the experiment, red-eyed white pigs never produced pigment, but dark-eyed white pigs did so regularly. In all cats the infection, as in the dogs, was accompanied with a mild inflammation.

Discussion

The data presented concerning *M. lanosum* infection are not statistically sufficient to make final recommendation as a method for testing antifungal remedies. It appears highly suggestive, however, that newborn rabbits seem to be suitable for the testing of antifungal remedies. This recommendation is based on the minimal individual variation of the clinical signs among all the animals investigated, and also because the time of fluorescence varies only slightly from the time of positive culture

findings. A test which, in this instance, would reduce the minimal positive cultural findings from 100 days to 50 days, would definitely be of value in assessing the antifungal value of a drug. Despite the fact that the Siamese kitten showed the longest time of fluorescence, as well as positive cultures, it is not recommended that this animal be utilized for the screening of antimycotic remedies, because they are too expensive and difficult to handle and to maintain.

Summary

(1) The clinical course, fluorescence, and mycological findings, of experimental *M. lanosum* infection in rabbits, newborn rabbits, cats, kittens, dogs, and puppies is described.

(2) The infection in nine-week-old rabbits showed great individual variation and contrast to infection in newborn rabbits. In the rabbits inoculated 24 days and 10 hours after birth the time of fluorescence and the cultural findings are rather uniform. A peculiar feature in both groups of these animals is the gradual shifting of fluorescence to the tips of the hair.

(3) The newborn rabbits infected with *M. lanosum* showed minimal individual variation. These animals therefore, appear suitable for screening of antifungal remedies, and are recommended for such use.

(4) Dogs infected with *M. lanosum* culture showed a stronger response than those infected with the *M. lanosum* infected hair. The mean fluorescence was uniform in all the dogs. Positive cultures could be recovered for 133 days in one of the mongrel dogs, and for 112 days in a Chihuahua puppy. Alopecia in these animals is also not considered as a clinical sign.

(5) Siamese cats and kittens, as well as alley kittens, showed a rather widespread progressive infection. In some animals the perioral area was heavily infected, but the tactile hair remained free from the infection. The female Siamese kitten showed the longest period of fluorescence (251 days).

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CONFERENCES HELD

SECTION OF BIOLOGY

"RESERPINE (SERPASIL) AND OTHER ALKALOIDS OF *RAUWOLFIA SERPENTINA:* CHEMISTRY, PHARMACOLOGY, AND CLINICAL APPLICATIONS"

FEBRUARY 5, 1954

The Section of Biology held a conference on "Reserpine (Serpasil) and other Alkaloids of *Rauwolfia serpentina:* Chemistry, Pharmacology, and Clinical Applications." Doctor Fredrick F. Yonkman and Doctor Frank L. Mohr, Research Department, Ciba Pharmaceutical Products, Inc., Summit, New Jersey, were the Conference Chairmen in charge of the meeting.

The program consisted of the following papers:

Friday, February 5

Morning Session. Chairman: Robert W. Wilkins

"Opening Remarks," M. J. Kopac, Chairman Section of Biology, The New York Academy of Sciences; Department of Biology, New York University, New York, N.Y.

"Chemistry of *Rauwolfia* Alkaloids, including Reserpine," Emil Schlittler and Harold B. MacPhillamy, Research Department, Ciba Pharmaceutical Products, Incorporated, Summit, N.J.

"Pharmacology of *Rauwolfia* Alkaloids, including Reserpine," Albert J. Plummer, A. Earl, J. Schneider, J. Trapold and W. Barrett, Research Department, Ciba Pharmaceutical Products, Incorporated, Summit, N.J.

"Endocrine Aspects of the Pharmacology of Reserpine," Robert Gaunt, A. Renzi, N. Antonchak, G. J. Miller and M. Gilman, Research Department, Ciba Pharmaceutical Products, Incorporated, Summit, N.J.

"Clinical Usage of *Rauwolfia* Alkaloids, including Reserpine (Serpasil)," Robert W. Wilkins, Boston University School of Medicine, Boston, Mass.

"Clinical and Experimental Effects of Reserpine (Serpasil) in Hypertensive Patients," Edward Freis, Georgetown University School of Medicine, Washington, D.C.

"Observations upon Intravenous Administration of Reserpine (Serpasil) in Patients with Essential Hypertension," Joseph Hafkenschiel and Alfred M. Sellers, University of Pennsylvania School of Medicine, Philadelphia, Pa.

Afternoon Session. Chairman: Irvine H. Page

"Reserpine (Serpasil) and the Alseroxylon Alkaloids of *Rauwolfia serpentina* in Hypertension," Travis Winsor, University of Southern California School of Medicine, Los Angeles, Calif.

"Observations of Cardiovascular and Renal Hemodynamic Responses to Reserpine (Serpasil) and Clinical Results in Treatment of Hypertension," John Moyer, Baylor University School of Medicine, Houston, Texas.

"Clinical Use of Reserpine (Serpasil) in Geriatrics," Raymond Harris, Albany Medical College and Ann Lee Home, Albany, N.Y.

"Studies of Reserpine (Serpasil) in Neuropsychiatric Patients," Nathan S. Kline, Rockland State Hospital and Columbia University College of Physicians and Surgeons, New York, N.Y.

Use of Reserpine (Serpasil) in Certain Gynecologic Disorders," Robert Greenblatt, Medical College of Georgia, Augusta, Ga.

"Clinical Experience with Reserpine (Serpasil) – A Controlled Study." Harriet P. Dustan, R. Taylor, A. C. Corcoran and I. H. Page, Research Division, Cleveland Clinic, Cleveland, Ohio.

"THE RELATION OF IMMUNOLOGY TO TISSUE HOMOTRANSPLANTATION"

FEBRUARY 12 AND 13, 1954

The Section of Biology held a Conference on "The Relation of Immunology to Tissue Homotransplantation." Doctor John Marquis Converse and Doctor Blair O. Rogers, New York University College of Medicine, New York, N.Y., were the Conference Co-Chairmen in charge of the meeting.

The program consisted of the following papers:

Friday, February 12

GENERAL DISCUSSION OF IMMUNOLOGY: SYSTEMIC AND LOCAL FACTORS

Morning Session. Chairman: E. J. Eichwald

"Introduction," M. J. Kopac, Chairman, Section of Biology, The New York Academy of Sciences; Department of Biology, New York University, New York, N.Y.

"Opening Remarks," John Marquis Converse, New York University College of Medicine, New York, N.Y.

"Tissue Response in Immunity," Reuben L. Kahn, University Hospital, University of Michigan, Ann Arbor, Mich.

"The Allergic Response and the Tuberculin Reaction," Robert A. Cooke. The Roosevelt Hospital, New York, N.Y.

"Compatibility and Non-compatibility," Harry S. N. Greene, Yale University School of Medicine, New Haven, Conn.

"The Role of Wound Healing in Relation to the Tissue and Humoral Responses in Homotransplantation," F. B. Engley, Jr., M. Allgower, and C. Snyder, University of Texas–Medical Branch, Galveston, Texas, and Burgerspital, Basel, Switzerland.

"Reticulo-endothelial Activity During Antibody Response," Elemer R. Gabrieli, Yale University School of Medicine, New Haven, Conn.

TECHNIQUES FOR DETECTION OF IMMUNE RESPONSES

Afternoon Session. Chairman: John H. Mulholland

"Observations on the Circulation of Homologous Skin Grafts *in Vivo*," H. Conway, R. B. Stark, and J. D. Sedar, Cornell University Medical School, New York, N.Y.

"The Definition of 'Survival Time' in Skin Homotransplants," A. Cecil Taylor, New York University School of Dentistry, New York, N.Y.

"Studies on the Localization of Radioactively Labeled Specific Gamma Globulin in Skin Homotransplantation," Mario Gaudino, New York University College of Medicine, New York, N.Y.

"The Antibody Response to Skin Homotransplantation in Mice," Peter A. Gorer, Guy's Hospital, London, England.

"Tissue Localizing Antibodies," David Pressman, The Sloan-Kettering Institute for Cancer Research, New York, N.Y.

Saturday, February 13

METHODS OF MODIFYING THE HOST RESPONSE

Morning Session. Chairman: George W. Hyatt

"The Effect of Single and Multiple Injections of Skin Antigen on the Survival of Homologous Skin Grafts," Creighton A. Hardin and Alvar A. Werder, University of Kansas Medical Center, Kansas City, Kans.

"Induced Alteration of the Normal Host-Graft Relationship in Homotransplantation of Mouse Tumors," Nathan Kaliss, Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Me.

"Growth of Human Tissues in Cortisone Treated Laboratory Animals," Helene Wallace Toolan, Memorial Center for Cancer and Allied Diseases, New York, N.Y.

"Homologous Implantation of Kidneys in Pseudo-Parabiotic Rats," S. Lang, G. J. Dammin, and B. F. Miller, Harvard Medical School and Peter Bent Brigham Hospital, Boston, Mass.

"Acquired Tolerance of Foreign Homologous Cells," R. E. Billingham, L. Brent, and P. B. Medawar, University College, London, England.

IMMUNE REACTIONS IN TISSUE HOMOTRANSPLANTS

Afternoon Session. Lyndon A. Peer

"The Immune Concept: Its Relation to Bone Transplantation," Michael Bonfiglio and Wayburn S. Jeter, State University of Iowa, Iowa City, Iowa.

"Immunologic Factors in Homogenous Bone Transplantation: I. Serological Studies," Paul H. Curtiss, Jr., and Charles H. Herndon, Western Reserve University School of Medicine, Cleveland, Ohio.

"Homotransplantation of Ovarian Tissue," Peter L. Krohn, University of Birmingham, Birmingham, England.

"The Acquired Immunity Concept in Kidney Homotransplantation," Morten Simonsen, Institute for Pathological Anatomy, University of Copenhagen, Copenhagen, Denmark.

"The Immune Concept: Its Relation to Corneal Homotransplantation," A. Edward Maumenee, Stanford University School of Medicine, San Francisco, Calif.

The Section of Biology provides conferences for active workers in special fields of Biology. Attendance is limited to those invited to participate in these conferences and to interested Members of the Academy.

NEW MEMBERS

Elected February 25, 1954

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